VOL. 48, No. 1 JANUARY, 1941

THE PSYCHOLOGICAL REVIEW

OPERATIONISM AND THEORY IN PSYCHOLOGY [1]

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I

During the last decade psychologists have become increasingly aware of the methodological problems of their science. This trend of thought has been but a part of the larger movement of Scientific Empiricism, which in turn gained momentum through the integration of the ideas developed in England (Russell) and on the continent (the Vienna Circle) with the pragmatist tradition in this country. In psychology, as in other sciences, these methodological analyses have stressed two different aspects of scientific enterprise. A number of psychologists, Tolman (17), Skinner (13), and Stevens (14), stimulated by the writings of Bridgman (3, 4) have centered their efforts largely on the *empirical component* of scientific method. Under the watchword of operationism, they have carefully considered and laid down the requirements that scientific concepts must fulfill in order to insure testability and thus empirical meaning. The second aspect, the *formal (theoretical) component* of scientific endeavor has been brought to the forefront in psychology principally through the writings of Hull (6, 7, 9) and Lewin (11, 12). By his persistent attempts to fit articulate theoretical structures to certain realms of behavioral data, the former has tried to show the essential rôle that formal systematization plays in scientific explanation. The latter has likewise emphasized the rôle of

[1] This article is an elaboration of two papers read by the authors in a symposium on Current Aspects of Behavior Theory at the 1940 meeting of the Midwestern Branch of the American Psychological Association in Chicago.

theory in his writings but has not as yet attempted rigorous theory construction.

In the broader framework of Scientific Empiricism the complementary nature of these two components of scientific method has been clearly recognized. It might even be said that one of the main achievements of this philosophic movement has been the methodologically correct evaluation and allocation of the respective rôles of these two modes of scientific thinking. Unfortunately in psychology there has not always been a proper appreciation of the incompleteness of an approach which neglects either one of these supplementary procedures in science. The present paper deals with some of the problems which have arisen in psychology with attempts to develop these two aspects of scientific method.

II

No body of empirical knowledge can be built up without operational definition of the terms in use. This basic methodological requirement is clearly recognized by the present writers. We should like, however, to call attention here to a certain tendency to use this 'operational criterion' illegitimately as a means of criticizing theoretical attempts which are, at least, *methodologically* sound. Very often the real basis of such criticism has been nothing but a healthy skepticism as to the scientific value of the theories under examination. There should, of course, be no hesitation at voicing such doubts whenever the generalizations of a scientist seem overdrawn or if his theories do not look promising. But to express such doubts as a methodological argument only confuses the issue. Even sterile or rash theories should be eliminated by proper means, for otherwise there is the danger that promising theories will also be ruled out for no good reason at all. At the present state of the discussion, therefore, it might be useful to restate rather carefully the limits and the legitimate scope of operational analysis.[2]

[2] Stevens' recent paper (15) on the subject is distinguished by a very cautious and circumspect attitude in this respect. The paper's main concern, however, is to integrate the methodological discussion within psychology with modern philosophy of science, a subject taken up by one of the present writers (1, 2).

Even before a scientist can set out to study some aspects of the phenomena he is interested in, *e.g.*, animal maze learning or human rote learning, he must have at least some tentative ideas as to what the 'relevant factors' might be, that is to say, as to what the determinants of the phenomena studied are. These ideas, of course, are drawn from his knowledge and his theoretical frame of reference. In a more advanced state, if such a relevant factor has become quantifiable, it is called a variable. Sometimes the term 'condition' is also used in this context, and we speak of the conditions under which the phenomena occur. One important point that must be made here is that there is no methodological principle, no 'operational recipe' which guarantees that no relevant factor has been overlooked. A statement such as this, then: Operationism requires that all the conditions be taken into account, can be quite misleading. For a construct which unwittingly leaves out a relevant factor (or determining condition) just leads to a different formulation of the empirical laws. For example, by telling us what manipulations he performs, what pointers he reads (weighing and measuring cubic content), and what computations he carries out with the numbers thus obtained (division, W/V), a primitive physicist would give us a methodologically correct definition of his *empirical construct* 'density of a liquid.' And this in spite of the fact that he might not have given any attention to the temperature in his laboratory, one of the conditions upon which, as we know, and as he might not know at that state of his investigation, the result of his manipulations and computations depends. The point is that we are able to trace back the terms of his language to the immediately observable. He has laid down all the conditions under which he is going to say: "This liquid has the density 1.3." Therefore, we know what he means, and that is all general methodology can insist upon at this level of the so-called *operational definition of empirical constructs.*

Historically and psychologically, then, the creation of helpful concepts is a very essential part of a scientific achievement. From the standpoint of methodological analysis, however, if the scientist has defined his concepts, he has only

prepared his tools. Explanatory work proper starts, and can start, only after the empirical constructs have been laid down. And this work, of course, consists in nothing else but finding the *empirical laws, i.e.*, the functional relationships between the variables. 'Finding' in this context means inductive generalization from observation and experimentation. In this way, our hypothetical physicist, for instance, might become aware of the fact that the density of liquids, as defined by him, varies with temperature. He will then set out to establish the functional relationship between these two variables. But whether a more complete law would have to be established between his two variables and the further one of pressure, is again a pragmatic and no methodological question. We add two further remarks in elaboration of this cardinal point.

First, it must not be overlooked that many empirical laws consist just in the finding that different 'classes of operations' lead, within certain ranges, to the *same* result. The operations are then said to define the *same* empirical construct. The classical example for such an empirical law is furnished by the alternative ways of measuring length either with a yardstick or by triangulation. Operational analysis has made us alert to the tentative and empirical character which such 'identification,' often uncritically and sometimes unjustifiably assumed, shares with any other empirical finding.[3]

Secondly, suppose that the scientist of our example, still ignorant of the relation between temperature and density, desired to ascertain whether his empirical construct 'density' could be employed as an identifier of liquids. Upon discovery of the fact that the 'same' liquid gives different 'density-values' under different temperatures he would realize that this cannot be done. But then he might try to identify liquids by recording their density at a given standard tem-

[3] In order to comply with the more rigorous distinctions of Scientific Empiricism, one should speak here of 'reduction chains' (Carnap) rather than of 'classes of operations.' A further prosecution of these points, however, leads directly into the investigation of the rules governing our use of 'thing-names' and of the spatio-temporal frame of reference. Fascinating as this problem is, it does not seem that methodological clarification in psychology needs to go so far back into epistemology and logical analysis proper; the less so, as practically all psychologists agree that only physical phenomena are the material that psychology, like any other natural science, is concerned with.

perature. Strictly speaking, this 'standard density' is a new construct.[4] The point is that, even if this new construct proves satisfactory for our physicist's present purposes, he has not yet stated all the conditions simply because there is always, at least theoretically, an infinite number of them. He might, however, have got hold of all the factors relevant for his purpose: *i.e.*, within the range of specification and variation determined by his experiments and their intended generalization. The proof of the pudding is in the eating and not in any particular operational criterion. We see that even at the level of the empirical laws the scientist cannot derive any help from operationism. He will have to rely upon his own ingenuity and whatever help he might be able to get from an articulate theory.

Having formulated this limitation of operationism so strongly, it is only fair to state the real and sound scientific basis of the demand for careful consideration of 'all the conditions.' In the less complex and more mature fields of natural science (physics, chemistry) we are reasonably confident that we know and control *practically* all the variables necessary for the complete functional description attempted. In the biological and social sciences, on the other hand, this is not the case. Here, complexity of the situation and insufficiency of knowledge tend to preclude successful segregation of a set of variables which is reasonably complete. This is a shortcoming which limits the importance of most of the 'empirical laws' and of theorizing in these fields at their present stage of development. The difficulty should be clearly recognized and it is to the credit of the operationists in psychology that they insist upon ultra-cautiousness and skepticism in these matters.

[4] It is worth while mentioning, though, that these two constructs are in an hierarchical relation, *i.e.*, that the class of operations (reduction chain) leading to one of them is a subclass of that leading to the other. The same holds true for concepts in psychology like learning criterion and retention criterion. There is, of course, a manifold factual interdependence between constructs and laws. Still we believe that the sharp analytical distinction between empirical constructs and empirical laws made in this paper is justifiable within scientific methodology, and might prove especially helpful for a clear appreciation of the operational viewpoint in present day psychology. At the level of logical analysis, however, the disentanglement of the thoroughgoing interdependence between the terms and the whole system of hypotheses constitutes one of the main tasks of scientific philosophy.

But again it must be emphasized that the difficulty is not a methodological one. The question whether the individual variables are adequately and properly defined should be carefully distinguished from the question as to whether a set of variables sufficiently complete for a satisfactory functional description has been ascertained.

Before turning to the discussion of theory, there is still one further point to be made regarding the definition of 'empirical constructs,' the only aspect with which, as we have seen, operationism is properly concerned. The language of any science contains a whole hierarchy of interlocked empirical constructs—mass, acceleration, momentum, energy, or stimulus trace, excitatory potential, and so on. None of these particular constructs, of course, is 'observable,' in the sense in which a physical thing is observable. Nevertheless they are just as empirical as length, duration, weight, stimulus and all other such terms as are sometimes exclusively thought of as being operationally defined. All scientific terms are derived terms, derived from and retraceable to what one might call 'the hard data,' the 'immediately observable,' or what Stevens calls the 'elementary operation of discrimination.'[5] Any attempt then to divide this hierarchy of constructs into sheep and goats, *i.e.*, operational constructs and theoretical constructs, is of necessity arbitrary. Actually much of what is usually called theorizing in empirical science consists, as will be discussed later, in the creation of these organizing empirical constructs during the search for the empirical laws. And for this reason some of them are sometimes referred to as theoretical constructs. This is all that can be meant by this distinction.

III

Turning now to the discussion of the theoretical aspects of scientific method in psychology, one of the most important tasks would seem to be the clarification of the terms 'hypo-

[5] Again, in the stricter language of Scientific Empiricism, all these expressions are rather objectionable and would have to be replaced by the syntactical term 'primitive predicates.' On this level of analysis the names of physical things themselves can be considered as 'derived terms.' For all these problems Carnap's *Testability and Meaning* should be given as a general reference (5).

thetico-deductive method' and 'postulational technique.' A certain amount of confusion is apt to arise, and indeed has arisen, from an ambiguity in the meaning of these terms. In logic and mathematics they have reference to a formal language system developed as the consequence of a basic set of relations (called postulates or implicit definitions) between otherwise undefined terms. Hilbert's Axiomatics is an outstanding example of such a formal system. Interpretation by means of co-ordinating definitions relating the formal terms with empirical constructs makes this method a feasible one for scientists.

That is to say, the theoretical scientist can start from a set of undefined terms, a, b, c, state his postulates (implicit definitions) which relate them, and then show that by virtue of these few postulates, the terms a, b, c themselves or certain compound terms x, y, z, defined by means of them, fulfill exactly the formulæ which represent the empirical laws. This formal system may then be 'interpreted' by co-ordinating a basic class of empirical constructs (experimental variables) either to the original terms a, b, c (phenomenological theory), or to the compound terms x, y, z (non-phenomenological theory). Actually there are few, if any, instances of such a method being exclusively relied upon in the development of the empirical sciences. As a matter of fact, even in geometry, the postulational method was a late achievement, born out of the need for systematic organization and epistemological clarification.

In actual scientific practice and particularly in recent psychological discussions (Hull) the term 'mathematico-deductive' has been used in a meaning different from that of the formal logico-geometrical term just defined, although the distinction has not always been apparent. As the scientist usually understands the term, mathematico-deductive method consists in making guesses or hypotheses as to the choice of constructs (variables) and the mathematical relationships holding between them, and the further notion, not always followed in actual practice by psychologists, of strict and complete deductive elaboration of the consequences implied

in these assumed relationships. Obviously, such a procedure is not necessarily hypothetico-deductive in the first sense.

This distinction is especially important for a proper understanding of the theoretical systems put forward by Hull, which we will therefore use as an exemplification of the general principles laid down in the preceding paragraphs. One can find the most divergent statements concerning various aspects of Hull's theories. Thus one writer (**12**) states that Hull's terms (concepts) are more or less well defined operationally, but that they are lacking in conceptual properties. At the other extreme one hears that his constructs are too highly theoretical and completely lacking in any empirical reference. These conflicting points of view appear to have arisen from Hull's terminology, particularly from his use of terms like 'mathematico-deductive,' 'undefined concepts,' and 'postulates.'

Misunderstanding might have been avoided if there had always been clear recognition of the fact that Hull's theorizing is hypothetico-deductive only in the second meaning outlined above. Hull does not begin with a set of purely formal terms, having no other meaning than that imparted to them by a set of implicit definitions, from which are then derived new terms and theorems made testable by means of co-ordinating definitions. Instead he actually begins with terms directly operationally defined. Unfortunately, he called them 'undefined concepts,' and thus created the erroneous impression that he started with purely formal terms which are never given the necessary co-ordinations to empirical constructs. A careful examination of these so-called 'undefined concepts,' however, will show that they are nothing but what one would call in a less sophisticated language the basic experimental variables, *i.e.*, the variables manipulated and observed in the laboratory.[6]

[6] In the recent monograph on rote learning Hull (**9**) uses the terms 'undefined concepts' and 'definitions' (defined concepts). Both undefined and defined concepts consist largely in what might be described as directly operationally defined concepts and there is no essential methodological difference between them. Apparently the idea underlying Hull's distinction between these two categories is that the undefined concepts are those most directly point-at-able, *i.e.*, involve the shortest defining sentences. Having selected these, he employs them in the definitions of the more complex

The essential point to be noted, however, is that Hull's postulate systems do not contain implicit definitions relating these initial variables (concepts). Indeed, his postulates are nothing but definitions which define new constructs by stating them as mathematical functions of the initial variables (concepts). For example, in a recent modified formulation of his theory of simple adaptive behavior, Hull (**8**) gives as a postulate, what is really a definition of his term stimulus trace ($\mathfrak{s}$):

$$\mathfrak{s} = a \log S(1 - e^{-hT}) e^{-kT'}, \tag{1}$$

where S is the intensity of the physical stimulus; T the time of duration of S; T' the time since the termination of S; a, h, and k being empirical constants. Here the term stimulus trace has been newly created out of the independent variables S, T, and T' by means of a mathematical technique.[7]

Several questions now arise concerning the scientific status of such 'postulates,' the answers to which will reveal a characteristic feature of psychological theory. Are these formulæ guesses as to the empirical laws of psychology? Obviously not in the same sense as the gas law or Newton's attraction formula, for there is no independent empirical referent, so far, for the newly defined variable. What then is the possible use of such *a priori* constructions?

For a satisfactory answer to this latter question we must examine the basic task of psychology. Like every other science, psychology conceives its problem as one of establishing the interrelations within a set of variables, most characteristically between response variables on the one hand and a

defined notions. The whole system of definitions and postulates exhibits the mixture of explicit definitions and reduction chains characteristic of empirical science.

It should be mentioned that U6, U7 and U8 should not have been included among the undefined concepts of the rote learning monograph. They are elucidations regulating the use of derived terms defined later in the postulates. This has been recognized by Hull, himself, in a later section of the monograph (p. 306). By and large, however, operationists will correctly interpret Hull by substituting 'operationally defined' for his expressions 'undefined concepts' and 'definitions.'

[7] It should be clearly understood that, in spite of the use of such terms as stimulus trace, excitatory potential, etc., no physiological referents for these arbitrarily defined terms are implied. Attention should also be called to the point that by this very procedure these derived terms become themselves empirical constructs, or if one prefers, they are indirectly operationally defined. They are methodologically comparable to the physicist's concepts of momentum, energy, etc.

manifold of environmental variables on the other. Or, in the usual mathematical denotation:

$$(2) \qquad \begin{aligned} y_1 &= f_1(x_1\, x_2 \cdots x_n) \\ &- - - - - - - - - \\ y_m &= f_m(x_1\, x_2 \cdots x_n) \end{aligned}$$

The problem here is two-fold: (1) the obtaining of the empirical curves and (2) the determination of their mathematical form, *i.e.*, the specific nature of functions $f_1, f_2 \cdots f_m$. In solving this problem, physics is able to start out with assumptions as to the specific form of the f's describing elementary situations, *i.e.*, situations of simple structure with a very limited number of variables, for it is possible to generalize or hypothesize these functions from experimental observation. More complex situations can then be adequately handled by deduction from and combination of these basic formulæ. In the course of this elaboration the physicist sometimes finds it convenient to employ new auxiliary terms (*e.g.*, force in mechanics) defined out of the original variables.

In psychology, on the other hand, the number of variables entering into even the simplest behavior situation that can be experimentally produced is so great and the structure of their interrelationship is so complex that we are unable to make even a first guess as to the mathematical form of the equations directly from the empirical data without some auxiliary theoretical device.[8] *The terms defined by Hull's postulates provide just such a device.* They attempt to bridge the gap between the two sets of variables, those manipulated by the experimenter and those measuring the observed responses. Technically, they aim at providing the means for ascertaining a rational fit to the empirical curve.

This aspect of our analysis can perhaps be best illustrated by a brief consideration of Hull's most recent formulation of simple behavior theory (**8**). The figure attempts to give a graphic representation of the hierarchic order of the terms involved. The basic (directly operationally defined) vari-

[8] The securing of empirical data under concomitant variation of several variables is only beginning in psychology.

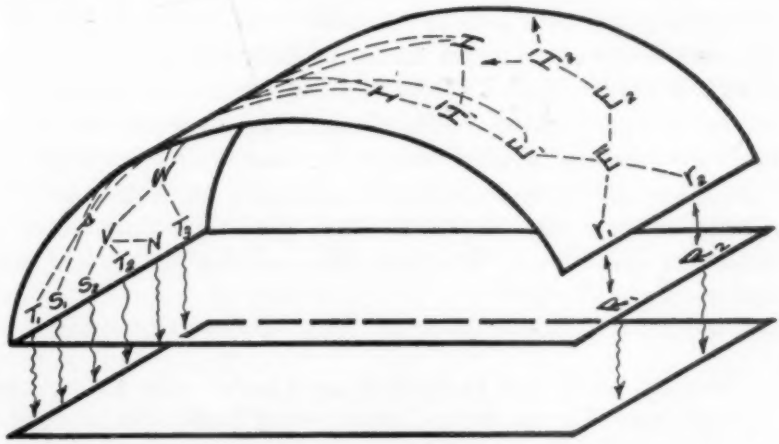
ables from which the construction starts, as shown at the left, are:

T_1 = the time of duration of S_1,
S_1 = the intensity of a physical stimulus, *e.g.*, buzzer, lever,
S_2 = reinforcing (goal) stimulus, *e.g.*, food,
T_2 = starvation time,
T_3 = time between response to S_1 and occurrence of S_2,
N = number of presentations of $S_1 - S_2$ sequence, *i.e.*, trials.

Moreover there are the various measurable aspects of the response shown at the right:

R_1 = amplitude of response,
R_2 = latency of response, etc.

Hull now proceeds to the task of specifying the mathematical form of the empirical laws relating these two classes of variables. He attempts to do this by defining new terms out of the original variables so that in the final formulation of the empirical laws only the end members of this chain of intervening variables appear. This is accomplished in the manner schematically illustrated in the figure. The dotted



lines indicate that these intervening variables, shown on the bridge, are derived in stepwise fashion from each other. To go into any detail would be beyond the scope of this paper. It is sufficient to indicate here that the final rational equations

would express the response variables as a function of the just preceding intervening variable, excitatory potential ($\bar{E}$). It will also be noted that the response variables appear twice in the diagram; once on the bridge as derived, intervening constructs (r_1 r_2) and once as independently operationally defined variables (R_1 R_2).

These two sets of formally different terms (r's and R's) are then identified and the success of the construction depends upon whether this identification is borne out by the experimental data. If so, the gap at the right end of the bridge is closed and the desired formulation of the empirical law has been attained. Finally, to complete the description of the diagram, the wavy lines leading down to the base symbolize the completion of the operational tie-up with the 'immediately observable.' [9]

For the sake of methodological completeness attention should be called here, perhaps, to a point of considerable importance (one somewhat obscured by the present emphasis) in regard to the determination of the final and complete rational form of the empirical law. By 'final and complete' is meant the precise specification of each and every function connecting the two sets of variables. However, many deductions as to shapes and trends of behavior curves can be drawn if only some of the mediating functions (postulates) are specified, provided that the remaining non-specified functions hold to the general pattern assumed. As a matter of fact, many of the testable deductions made by Hull from this particular set are of this kind, for so far he has only roughly indicated the type of functions beyond the term H in the above scheme.

IV

In concluding this analysis it may be of some interest to indicate how this theoretical structure of Hull's relates to the conception of the nature of psychological theory outlined by Tolman in his presidential address (**18**) and earlier papers

[9] This latter feature of the diagram follows the method of graphical representation suggested by Feigl and Koch in their clarification of the relation between empirical and formal language in psychology (10).

(**16, 17**). Tolman begins with equation system (2) and like Hull and any other methodological behaviorist conceives the task of psychology as one of determining the form of the functions (see page 12). In his scheme the independent variables ($x_1 x_2 \cdots x_n$) are split into two classes, environmental and individual-difference variables, and the dependent variables are some aspects of behavior. As to the form of the functions, he states that "It is in fact so complicated that we at present seem unable to state it in any single simple statement. We find, rather, that we have to handle it by conceiving it as broken down into successive sets of component functions. These component functions connect the independent variables to logically constructed 'intervening variables,' and then connect these intervening variables in their turn to one another and to the final dependent behavior" (**17**, p. 91).

Clearly this description is fully in line, indeed identical, with the picture of Hull's actual procedure as outlined by our preceding analysis, and it is to Tolman's credit that he has been one of the first in psychology to outline this general methodological scheme. His actual theorizing, however, has always moved on a very general, programmatic level and has not gone beyond suggesting and cataloguing the various possible intervening variables and showing how they provide for the definition and use of mental terms (demands, hypotheses, etc.) in a behavioristic psychology. Such essential convergence between two theoretical viewpoints which are often regarded as being far apart strongly suggests that essential agreement on this level of general methodology is about to be reached in psychology. The most imperative task would now seem to be the persistent pursuit of attempts to formulate articulate theories in closest contact with the experimental data.

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[MS. received June 26, 1940]

THE LOGICAL CHARACTER OF THE MOTIVATION CONCEPT. I[1]

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I. Introduction

Psychology is at present in the throes of a far-reaching methodological renaissance. Analysis of basic presuppositions, discussion of foundation problems, formulation of the logical principles inherent in theoretical construction has never before been cultivated with such furious intensity by psychologists. Terms like 'operationism,' 'hypothetico-deductive procedure,' 'postulate system' have become ubiquitous in recent theoretical discussion. Almost every psychology sophomore knows that it is bad form if reference to 'definition' is not qualified by the adjective 'operational'; and the demand for 'postulates,' 'derivations' and 'theorems' has already become a must in 1939 psychological dialectic. Testimonials to the new methodological renaissance are the articles of Stevens (**27**, **28**, **29**), Hull (**14**, **15**, **16**), Bills (**3**), Kantor (**17**), McGeoch (**23**, **24**), and Pratt's recent book (**25**), to list but a haphazard sample.

Psychology is in a position to profit immensely from the tools of analysis recently forged by logicians and philosophers of science; for bringing these tools to its attention it cannot be too grateful to its enthusiastic new methodologists. But the line between enthusiasm and evangelism is an ambiguous one, and among the motivational virtues of evangelism are not accuracy and perspective. It is only to be expected that in the first stages of such a methodological shake-down, ultimate precision in the introduction and interpretation of new

[1] This article comprises part of a thesis submitted in partial fulfillment of the Master's degree in the Department of Psychology, State University of Iowa, 1939. It owes its existence largely to the assistance and encouragement of Professor Herbert Feigl, under whom the thesis was written.

concepts will not always be in evidence. For purposes of popularization it may not even be desirable to cut lines too finely. But it is nevertheless true that some of the key notions which have recently been imported from philosophy of science have suffered distortion in the hands of their psychologist intermediaries; others have been given an importance and range of application which they do not merit. In many cases more rigorous formulation might have avoided difficulties.

In the light of this situation, then, it might be fruitful to take certain of the concepts which have figured so prominently in recent discussion out of the Platonic empyrean of Ideas in which, for the most part, they are still embedded, and see what they are worth in actual operation. For this purpose we have chosen 'operationism' and 'hypothetico-deductive procedure' as the cardinal notions to scrutinize. And we have chosen for the proving grounds of these devices a concept which can lay claim to the most extravagant vested interest in confusion of any in psychology—'motivation.' By an examination of certain aspects of the motivation concept with the aid of 'operational' and postulational technique,[2] we may hope: (1) to gain further insight into the utility and range of application of these procedures, and (2) perhaps incidentally dispel some of the confusion surrounding the motivation concept.

The double purpose of this paper may appear surprisingly convenient. Should the present status of the motivation concept not give way to clarity and order as a result of our analysis, the implication may seem left open that it is the fault of the tools and not the analysis. We will rule out this implication at the very outset. The analysis of motivation is primarily introduced for illustrative purposes; its function is largely to provide a locus of concreteness for the discussion of methodological issues.

Since Stevens (27) has recently called the attention of psychologists to the achievements of scientific empiricism,[3] it

[2] 'Postulational technique' will be used as a synonym for 'hypothetico-deductive procedure.'

[3] Also known as 'logical positivism' and 'logical empiricism.'

might be appropriate to clarify the relation between 'operationism' and 'postulational technique' and the work of that movement. It is an unfortunate circumstance that these two procedures were introduced to psychologists in different contexts and by different individuals. Hull and his followers have tended to emphasize the virtues of postulational technique, whereas the Stevens-Boring school has tended to place almost exclusive emphasis on operationism. Our thesis will be that operationism and postulational technique point to two necessary and complementary phases of theory building in the empirical sciences. Postulational technique, taken by itself, will generate only purely formal language systems; operational definition, taken alone, will make it possible to use terms meaningfully. Hitch the constructs appearing in your postulates to a field of empirical 'fact' and only then do you get a scientific theory. It is our contention that both operationism and postulational technique find their proper integration in the work of scientific empiricism—particularly its researches into syntax and meaning theory. This, we hope, will be made clear as we proceed with the discussion.

II. Postulational Technique

The Nature of Formal Systems.—Language systems fall into two classes: (*a*) formal, and (*b*) empirical. The field of formal language systems is coextensive with the field of logic and pure mathematics.

A formal system consists of (1) a *postulate set* (implicit definition), (2) a group of *explicit definitions* of certain terms [4] appearing in the postulates, (3) *rules of inference*, (4) *theorems*. A postulate can be any sentence which is asserted for the purpose of exploring its consequences. There are no restrictions on what may be chosen as a postulate.[5] The postulates assert certain relations holding between two or more symbols (terms). A formal system can get along without using ex-

[4] 'Term,' 'concept' and 'construct' will be used synonymously in the present paper.

[5] Carnap (9) has called attention to this point in his 'principle of tolerance.' It follows from this that an indefinite number of 'logics' are possible. Which postulates are chosen is always a function of the purposes of the postulator.

plicit definitions. But in most formal systems there appears in addition to the postulate set—which serves as an implicit definition of the terms appearing in it—a group of explicit definitions. These are simply rules stating that certain symbols may be substituted for other symbols occurring in the postulates. The rules of inference state how we may proceed from one sentence to another in making derivations. They define the notion of 'derivability' for the given system in which they appear. The theorems are the sentences that are derivable from the postulates and the explicit definitions by the aid of the rules of inference. They serve simply to make explicit what is already implicitly contained within the postulates.

Empirical Systems.—A formal system can be transformed into an empirical (scientific) system by the process of 'interpretation' of its postulate set. Interpretation consists in correlating to each term of the postulate set a term having some empirical reference. A term may be said to have empirical reference (scientific meaning) if there is a rule correlating it to some observable state of affairs, *i.e.*, if we can apply it according to the presence or absence of some set of observable symptoms.[6] We will call the correlation of an empirical term to a formal term occurring in a postulate set '*coordinating definition.*'

Looking at a formal system from the point of view of its scientific utility, we may characterize it as a kind of grinding machine, the function of which is to grind out the implications contained in the relationships defined by the postulate set. The unfortunately finite capacities of most mortals are adequate to the intuitive derivation of only a few of the simpler theorems contained in any complex postulate set. Given the formal system, however, we have only to turn the crank to get as many theorems as we want—provided we are sufficiently patient. Taken by itself, however, a formal system is an intricate tautology. Interpret that system with constructs

[6] This is a very loose and perhaps misleading first approximation to the criterion of scientific meaning. We shall discuss this question at length when we consider the reduction sentence, and operational definition.

from some scientific domain and—if we have found the right system and the right domain—we find ourselves in the possession of an invaluable scientific tool. For now we are able to grind out *experimentally testable* consequences from our empirical constructs which, before systematization, we might never have dreamed of. It is only through formalization of a theory by means of postulational technique that we can arrive unambiguously and exhaustively at its meaning.

The Interpretive and the Telescopic Procedures.—The above brief characterization of scientific systems represents a logical reconstruction of the processes involved in theory building, rather than an historically accurate description of how theories are brought into being. One never finds a formal system and a group of empirical laws which, by some preestablished harmony, happen to dovetail perfectly. In actual practice, interpretation involves extensive trial-error modification of the formal system and the empirical subject matter. Furthermore there is no standard procedure for setting up a scientific system. One may develop a formal system for the specific purpose of applying it to a given empirical domain, or one may employ some extant formal system which happens to 'fit.' Again, it might be convenient to elaborate the formal system and the conceptual network of the empirical subject matter simultaneously.

In general we may distinguish two types of procedure used in the construction of empirical systems. For want of better terms we shall call them the *interpretive* and the *telescopic* procedures. Of the two, the interpretive procedure adheres more closely to the steps outlined in our logical reconstruction. An investigator concerned with some special field of research, as yet in a fairly inchoate state, may have a 'hunch' that some already developed formal system (of pure mathematics) may serve as a fruitful tool for ordering the data of his field. He then proceeds to correlate, by means of coordinating definitions, certain of the constructs already occurring in his field to the terms appearing in the postulate set of the formal system. If the formal system really fits (*i.e.*, if it is *iso-*

morphic [7] with the empirical subject matter) every theorem deducible from the formal system will hold for the empirical domain to which it is correlated. Experimental verification of the theorems will tend to increase the probability of the now interpreted postulate set (theory).

The interpretive procedure is the one most commonly employed in physics. The only psychologist who has ever proceeded more or less explicitly along these lines is Kurt Lewin (**21**, **22**). On the originally intuitive assumption that the principles of mathematical topology serve admirably for the description of psychological processes, Lewin proceeds to coordinate certain psychological constructs to topological terms (*e.g.*, 'locomotion' to 'topological path').

The so-called telescopic procedure dispenses with the step of interpretation of an already existent postulate set through coordinating definition. Elaboration of the formal and empirical aspects of the system proceed simultaneously. The two steps of development of the formal system and interpretation are, in a sense, telescoped into one. In this case the scientist usually proceeds by asserting as postulates either certain empirical laws which appear in his field or prior assumptions as to the functional relationships holding between certain of the empirical constructs, or both. Experimentally testable deductions are then made which—if verified—serve to increase the probability of the assumptions comprising the postulate set. Such a procedure is rarely used in physics. In psychology, the miniature scientific systems (**15**, **16**) which Clark L. Hull has put forward are of this character.[8]

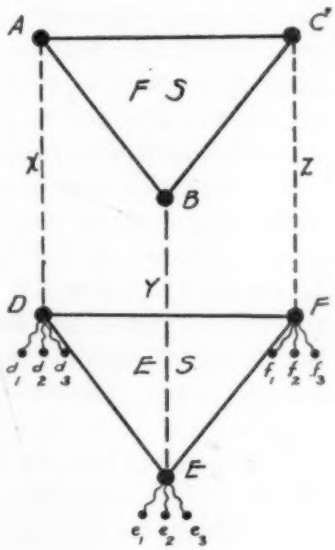
By way of summary we may say that the chief difference

[7] Two systems are 'isomorphic' if they are identical in structure. A rigorous but non-technical definition of the concept of 'structure' may be found in Russell's *Introduction to Mathematical Philosophy* (**26**, Chap. 6). Cohen and Nagel present an excellent short discussion of 'structural identity' or 'isomorphism' in *An Introduction to Logic and Scientific Method*, New York: Harcourt Brace, 1936, pp. 137–141. The logicians' 'isomorphism' should not be confused with Köhler's (**19**) brand.

[8] This difference between the logical procedure of Hull and Lewin has not previously been made explicit. Since the work of these two men represents the closest approximation to formal precision yet to be found in psychology, it is important to recognize the difference in their mode of attack.

between the interpretive and telescopic procedures is that the interpretation of some formal system as an explicit step in the process of theory building is included in the interpretive approach, while the telescopic procedure dispenses with this.

On Definition.—We have had occasion to refer to four types of definition which are employed in setting up formal and empirical systems. Let us summarize them. *Implicit definition* of a term consists of the properties assigned to it by the postulates in which it occurs. These properties have



the character of functional relationships connecting the given term with others. *Explicit definition* takes the form of a rule of substitution permitting the replacement of any occurrence of a given term appearing in the postulate set, by another. *Coordinating definition* correlates empirical constructs to the formal terms of the postulate set, and thus transforms an abstract system into an empirical one. *Operational definition* correlates the empirical constructs to some set of observable symptoms. The figure above illustrates the relationships between the basic types of definition. This may be understood

as an illustration of a simple scientific system [9] constructed by means of interpretive procedure. It consists of three postulates which relate three constructs. *FS* represents the postulate set (implicit definition) of the formal system. *ES* represents the postulate set of the empirical system which is built up by correlating empirical constructs *D*, *E*, *F* to formal terms *A*, *B*, *C*, through dotted lines of coordinating definition (*X*, *Y*, *Z*). Wavy lines of operational definition connect the empirical constructs *D*, *E*, *F* to their 'observable symptoms' (d_1, d_2, d_3, etc.).

Explicit definition has been omitted from the diagram not only to avoid complexity but also because explicit definition is not essential to the construction of a system. In the case of the above system the explicit definitions would be a set of rules stipulating that certain other terms may be substituted for formal terms *A*, *B*, *C* or empirical constructs *D*, *E*, *F*. The diagram for the telescopic procedure would omit, of course, formal system *FS* and the dotted lines of coordinating definition connecting it to empirical system *ES*.

It may be instructive to determine how and where the views of the chief proponents of rigorous theory in psychology may be fitted into the framework of our account of scientific systems.

It is obvious that the above characterization of scientific systems is akin to what Hull (**14**, **15**) calls 'hypothetico-deductive procedure.' There are two major differences between Hull's account and our own. First, he seems to identify hypothetico-deductive procedure with what we have called 'telescopic procedure' both in his general discussions of scientific theory, and in the 'miniature systems' which he has actually worked out. Secondly, Hull nowhere devotes sufficient attention to the problem of how the empirical constructs appearing in postulate sets are introduced (*i.e.*, the question

[9] It should be clear that the figure represents a system minus its theorems, *i.e.*, a postulate set. In order to perform deductions an additional set of elements is required. These are the 'rules of inference' ('transformation rules,' Carnap (**9**)) which specify the conditions under which we may proceed from one sentence to another in making derivations. In empirical systems such rules are presupposed but rarely stated.

of operational definition). The definitions which he advances in his 'miniature systems' (**14**, **15**) look very much like explicit definitions. This apparently has led Katsoff (**18**) to confront Hull with the fashionable accusation of not operationally defining his constructs. Examination of the Hullian definitions discloses that most are either operationally defined (albeit in a loose way) or definable; the rest are legitimate explicit definitions. Our complaint is not that Hull fails to meet the requirements of operational definition in his theoretical work, but that he fails to make explicit the place of operational definition in the framework of hypothetico-deductive procedure.

The 'operationists'—Stevens, Pratt, etc.—on the other hand, tend either to overestimate the importance of operational definition by an attitude of silence against postulational technique (or any of its equivalents), or to minimize the importance of postulational technique. The earlier articles of Stevens (**27**, **28**) conveyed the impression that theory building consists simply in the single step of operational definition, and that the operational definition of terms is a sufficient condition to fruitful scientific research. Pratt's (**25**) brand of 'operationism' admits that deductive procedure is desirable but suggests that psychology is not yet sufficiently mature for profitable formalization of its theories. Psychologists are exhorted to content themselves with the discovery of simple empirical laws, and the precise operational definition of the constructs which appear in them. At the other extreme, Adams (**1**) makes the comforting but somewhat broad statement that all fruitful psychological theories which have ever been put forward have employed hypothetico-deductive procedure.

Lewin (**21**, **22**) comes closer than any other psychologist to the present account of scientific theory in his discussions of methodological issues. In spite of many ambiguities and many points that require explication, we believe that his position can be made quite consistent with our own. His major source of confusion is that he uses the term 'coordinating definition' both in a sense corresponding to our own usage,

and as synonymous with operational definition. He defines 'coordinating definition' as follows (**21**, p. 213): "Certain observable facts are correlated to certain mathematical constructs." But we have seen that the 'observable facts' are not correlated directly to mathematical constructs, but are first correlated to empirical constructs and these in turn to mathematical constructs. When we break down Lewin's 'coordinating definition' into coordinating and operational components—and perform a few other acts of exegesis—we end up with a view of scientific theory essentially similar to the above.

The Nature of Explanation.—By 'explanation' in an empirical system we shall mean the following: If K is an empirical postulate set and S is a sentence derivable from K, then S will be said to be 'explained' by K and K will be said to constitute the explanation of S. Such a proposal might appear to delimit the class of explanations too stringently, but this objection vanishes when we realize that the limiting case of K would be a set of two postulates.

This view of scientific explanation has the following consequence, among others: S which is explained by postulate set K may in turn serve as explanation (or part of the explanation) of other sentences derivable from S. This principle implies that various *levels* of explanation may be distinguished in the sciences. The simplest type of explanation would consist of a universal proposition taken in conjunction with a particular proposition. Given the following:

$$(x)[P(x) \supset Q(x)] \text{ and } P(x)$$

we can say that $Q(x)$ is explained by these two sentences. Any student of elementary logic knows that Socrates is a mortal because all men are mortal and Socrates is a man. Trivial though the classic illustration of *Barbara* may seem, it is nevertheless the prototype of scientific explanation in its simplest form.

The fact that all men are mortal is in turn derivable from certain biological laws. These biological laws may themselves be derivable from certain laws of macro-physics. Thus we arrive at a hierarchy of explanations built up by relations

of deducibility. The simplest type of explanation may be called the level of empirical law; the successively more 'complex' levels might be called first order theory ($Theory_1$), second order theory ($Theory_2$) and so on.[10] It will be noted that this account of explanation equates scientific explanation with scientific theory.

Motivation as an Explanatory Construct.—'Motivation' is a term having many uses. Its manifold associations in the minds of most psychologists are shrouded in privacy. It is thought by some writers to be simply another label for 'psychology.' Others seem to feel that psychology can get on very nicely without ever using the term. A few of the indefinite number of applications that, at one time or another, it has had to its credit, are as follows: (1) a collection of phenomena comprising a sub-set of the phenomena of psychology, (2) a group of sentences describing these phenomena, (3) a process (*e.g.*, the process of initiation of activity), (4) a force, (5) an energy source, (6) a regulative and directive mechanism controlling energy expenditure, (7) a class of stimuli (external, internal or both).

The list could be expanded indefinitely, but cataloguing is not our purpose. It will immediately be seen that in (1) 'motivation' is used as the label for a set of facts; in (2) it is used as the label for a set of sentences; while in (3), (4), (5), (6) and (7) it is identified with some particular kind of construct the nature of which is defined by particular theories of motivation.[11]

It is in the context of 'motivation' as an historically given set of sentences that logical analysis is relevant. Actual examination of this set of sentences in the writings of psycholo-

[10] This terminology, as well as the substance of the above paragraphs, is taken from various unpublished writings of Herbert Feigl, who has for some time been elaborating a theory of explanatory levels in the sciences.

[11] Considerable, but unfortunately not all, of the confusion surrounding the use of the term has been generated by the failure of most writers to distinguish between these three modes of usage. More confusion is added by the tendency of those who have advanced special theories of motivation to reify the constructs defined by their theories. For instance, 'instincts' were considered by many psychologists to be 'real' psychic forces which impel an animal to action in much the same way as a tack on a chair impels to action.

gists discloses that the symbol 'motivation' occurs quite frequently. Statements of the following character are often found: "The animal did not run the maze because of improper motivation," "Learning rate is a function of degree of motivation." In these cases 'motivation' is meant to refer, ultimately, to some state of affairs. Clearly, motivation in this sense cannot mean the same as the 'motivation' that stands for the totality of sentences in which the term 'motivation' (or any of its synonyms) occurs. As a matter of fact these two usages belong to two different language systems. 'Motivation' as a set of historically given sentences is an expression of the so-called *syntax* language; while motivation as a term intending to have some objective reference is an expression of the *object* language. This distinction, as formulated by Carnap (**7**), is that between a language system referring to objective states of affairs (object language), and the language which describes the vocabulary and structure of the object language (syntax language).

We may now ask just what kind of term motivation is when it occurs in the object language. It is, in most cases, an 'explanatory' construct, serving as a conceptual link in attempted explanations of behavior. It follows from this and our view of explanation that the object-language usage of motivation necessarily involves a theory of motivation. And by theory, of course, we mean some set of postulates from which sentences describing the 'phenomena' of motivation can be deduced. That most theories of motivation are not put forward in postulational form largely accounts for the fact that motivation—like most 'explanatory' concepts in psychology—explains little.

Some psychologists (*cf.* Young, **32**) seem to have the impression that the concept of motivation has some esoteric monopoly on explanation in psychology. Such common forms of expression as "People perform actions *because* of a certain motive" have apparently led some psychologists to identify motivational psychology with 'explanatory psychology.' On the basis of our definitions, however, candidates for the title 'explanatory construct' must fulfill only one requirement:

they must appear in some empirical postulate set from which testable theorems may be deduced. Since postulates can be chosen at liberty, perfect democracy reigns among scientific constructs.

III. Operational Definition

Motivation a Disposition Concept.—We may now note another characteristic of the motivation concepts occurring in theories of motivation. They fall into the category of what Carnap has called disposition or capacity concepts. A disposition concept is one which denotes "a state characterized by the disposition to certain reactions" (**10**). Most high order scientific constructs are of this character.

The Reduction Sentence.—Carnap (**8**) has developed a very helpful device—the reduction sentence—which illustrates the manner in which concepts are introduced into the language of science. In this article Carnap's primary purpose is to place scientific empiricism's factual (scientific) meaning-criterion on a more rigorous and formal basis. The reduction sentence was developed in connection with the notions of reducibility and confirmability—in terms of which Carnap's new formulation of the meaning-criterion is stated. We will undertake the difficult task of presenting a simplified and non-formal resumé of these concepts.

We have already seen that the most general prescription of scientific empiricism as to scientific meaning is that a concept has meaning if there is a rule stating the conditions under which it may be applied. The concepts of 'reducibility' and 'confirmability' were developed for the purpose of making specific the form of these rules.

We begin with the notion of reducibility, or more fully, reducibility of confirmation. As a first approximation let us understand by confirmation some mode of deciding (verifying) empirical sentences consistent with the procedure of science. Then we may say: The confirmation of a sentence S is reducible to the confirmation of a class of sentences C, if confirmation of C entails [12] the confirmation of S. A sentence

[12] 'Entails' here simply means 'carries over to.' Carnap uses the formal notion of 'consequence' in this context.

consists of a predicate ('*P*') attributing some property to one or more individual arguments (*a*, *b*, *c*, . .). The sentence '*P*(*a*)' may be read "the space-time point '*a*' has the property '*P*.'" We may now give an analogous rule for reducibility of a predicate:[13] The confirmation of a predicate (*Q*) is said to be reducible to the confirmation of the class of predicates *C*, if every confirmation of a sentence in which a predicate of *C* appears, entails the confirmation of a sentence in which *Q* appears.

Now the confirmation of predicate *Q* might be reducible to the confirmation of class *C* of predicates, and the confirmation of the predicates of *C*, in turn, may be reducible to the confirmation of another class of predicates *D*, and so on. Where does this process of reduction end? Obviously, the reduction "must finally come to predicates for which we can come to a confirmation directly, *i.e.*, without reference to other predicates" (**8**, p. 456). It can be demonstrated that within certain limits the choice of such a 'reduction basis' is a matter of convention. Carnap makes the proposal that 'the observable physical predicates of the thing language' be taken as the reduction basis of the language of science. The thing language consists of simple sentences assigning observable properties to given space-time points or perceived physical things. Statements of the form: "The space-time point '*a*' is red," "'*b*' is hot," "'*c*' is heavy" are expressions of the thing language.[14]

'Confirmability' might now be defined in some such way as the following: A sentence is called confirmable if the confirmation of *S* is reducible to a class *C* of object predicates of the thing language. We see here that the notion of confirmability presupposes the notion of reducibility.

On the basis of considerations similar to the above, Carnap arrives at the following proposal for the criterion of factual

[13] Scientific constructs will be considered as predicates for the purposes of the present discussion.

[14] It might be mentioned that the much misinterpreted thesis of 'Physicalism' in the sense in which Carnap (**6**, **10**) has advocated it, is simply the assertion that every sentence of science is reducible to the predicate of some such language. We shall see later that in this sense the doctrine of Physicalism is nothing more imposing than the demand that every concept of science be operationally defined.

meaning: "Every synthetic (factually meaningful) sentence must be confirmable" (**8**, p. 34).

A 'reduction sentence' is a sentence by means of which we introduce a new predicate into the language of science. It may take the form of (1) an *explicit definition*, (2) a *conditional reduction sentence*. The class of conditional reduction sentences may be divided into *reduction pairs* and *bilateral reduction sentences*.

We have already dealt with explicit definition in connection with the nature of scientific systems. We must now note that an explicit definition can be not only a rule of substitution for a term appearing in a postulate set, but it may apply to any term appearing in any of the theorems as well. An explicit definition has the form: '*Definiendum*' ≡ '*Definiens*' where the *definiendum* is the expression to be defined; the '≡' is the sign for equivalence and the *definiens* is the defining expression. It is clear that the confirmation of the *definiens* would carry over in equal degree to the *definiendum*.

The large and very important class of disposition predicates are introduced by conditional reduction sentences, either the reduction pair or the bilateral reduction sentence. The reduction pair introduces a predicate (Q_3) into the language of science in sentences of the following form:

$$Q_1 \supset (Q_2 \supset Q_3)$$
$$Q_4 \supset (Q_5 \supset -Q_3).$$

Here Q_1 and Q_4 describe certain experimental conditions which must be fulfilled in order to determine whether or not a given space-time point has the property Q_3. Q_2 and Q_5 are the results which follow from the experiments. The formulae may now be read: "If experimental condition Q_1 is set up, then if Q_2 results the point has the property Q_3. If experimental conditions Q_4 are realized, then if Q_5 results the point does not have the property Q_3 $(-Q_3)$." Thus predicates Q_3 and $-Q_3$ are introduced in terms of predicates Q_1, Q_2, Q_4 and Q_5, and may be said to be *reducible* to these four predicates.

In some cases Q_1 will coincide with Q_4, and Q_5 with $-Q_2$. That is, the non-occurrence of experimental result Q_2 may be

a sufficient symptom for the absence of Q_3. We are then permitted to introduce Q_3 by means of the *bilateral reduction sentence:*

$$Q_1 \supset (Q_3 \equiv Q_2).$$

This may be read: "Given experimental conditions Q_1, then the point has the property Q_3 if and only if Q_2 results." Q_3 in this case is reducible to Q_1 and Q_2 ,while $-Q_3$ is reducible to Q_1 and $-Q_2$.

Both the reduction pair and the bilateral reduction sentence state a rule of application for the predicate which they introduce. But there are relatively few scientific concepts which are introduced by a single reduction pair or bilateral reduction sentence. Scientific concepts are usually correlated to a plurality of observable symptoms; indeed the number of such symptoms is often a reliable index to the fruitfulness of the concept. 'Temperature,' for instance, may be measured by a mercury column, an alcohol column, electrical resistance and so on. The meaning of a predicate, then, is the *totality* of reduction sentences which serve to introduce it.

If we can find the totality of reduction sentences which governs the application of a given term, may we not identify this set of reduction sentences with the explicit definition of the term? Since all definition is a matter of convention this may seem quite legitimate. Unfortunately, however, not all conventions are equally fruitful. Science is an ever expanding enterprise. And with the growth of science—the discovery of new laws, new techniques—scientific constructs are continually acquiring additional symptoms (reduction sentences). To arbitrarily rule out these possible future accretions by an act of explicit definition in terms of extant reduction sentences, is to play havoc with the normal metabolism of the language of science. Thus we find it expedient not to identify the totality of reduction sentences which a disposition predicate possesses at a given stage in the development of science with its explicit definition.

The meaning of a disposition predicate is never finally determined. Its range of application is conditional upon the specific experimental techniques (and their results) in terms

of which it is introduced. It is always possible that at some point in the future development of science, it may be found convenient to broaden its range of application by the addition of new criteria governing its usage.

Motivation and the Reduction Sentence.—We are now in a position to understand more fully what is implied by the dispositional character of the motivation concept. Motivation is certainly not a term that denotes something observable, *i.e.*, it is not defined by an explicit definition containing only observable thing predicates in the definiens. If this were the case, the present paper would be extremely short. When, for instance, we attempt to apply the statement: "The animal X (at time t) is more highly motivated with respect to goal A than with respect to goal B," it is futile to look for the 'motivation' sitting proudly somewhere on the animal. What we actually do, of course, is to look for certain symptoms of the motivated state. If goal A is food and B a mate, we might place X at the start of an alley with food and mate at the other end and observe which goal object he chooses. If the animal chooses food, then we feel justified in saying: "X is more highly motivated with respect to food than sex," or "X's hunger motivation is stronger than his sex motivation (at time t)." [15]

We can discover all the properties of the bilateral reduction sentence in the above commonplace example. Let Q_1 be 'placing X (at time t) at start of alley containing food and mate'; let Q_2 stand for "X chooses the food"; finally let Q_3 stand for the statement "X's hunger motivation is stronger than his sex motivation at time t." ($-Q_2$ will mean either that X chooses mate or X shows no interest in either goal object.) We may now write:

$$Q_1 \supset (Q_3 \equiv Q_2),$$

[15] We might, of course, substitute any of the synonyms for motivation in these expressions. 'Want,' 'need,' 'drive,' 'set,' 'determining adjustment,' 'propensity,' 'reaction tendency,' 'desire,' 'purpose,' 'demand,' 'valence,' 'disposition,' 'attitude,' etc., would do just as well in this context. The ratio of words to facts in this field is astounding. It should be clear that different symbols introduced by identical reduction sentences have the same meaning.

which may be read: "If we place X in the specified maze at time t, then X's hunger motivation is stronger than his sex motivation if and only if X chooses food." There are additional symptoms in terms of which the concept 'relative degree of motivation' may be introduced. X may be put in an obstruction box and the relative strength of his motivation with respect to different goal objects might be determined by the differential intensity of electric shock he will take in order to reach the goal, or the differential number of times he crosses the grid (shock being held constant) on his way to the goal object in a given period. The sum total of reduction sentences which can be specified for the predicate 'motivation' at a given stage in the development of psychology may be taken as the empirical meaning of the concept *at that time.*

When we can state the totality of reduction sentences for a scientific concept we shall say that it is *operationally defined.* There is no good reason for adding this extra term, except that its use will eliminate the need for some such term as 'reductive definition.' Since the notion of 'operational definition' has already been sold to psychology and since the vocabulary of psychology is already sufficiently rich, there is no point in putting a new synonym on the market. But before we blandly proceed to make this identification, we had better determine whether it is permissible, and whether what psychologists have been calling 'operational definition' stands to profit by it. And answering these questions will give us the opportunity to try to shed some light on the relationship between 'operationism' and scientific empiricism.

The Reduction Sentence and Operational Definition.—We have already noted the extent to which the fad of 'operationism' has pervaded the psychological atmosphere. Little attention has been devoted to the question of the relationship between operationism and scientific empiricism.[16] Since Stevens (29) and Pratt (25) have recently introduced scientific

[16] Stevens (29) summarizes some of the contributions of the movement but does no more than simply note that the meaning criteria of 'operationism' and scientific empiricism are in essential agreement. Pratt's treatment of the question can be inferred from the following chapter heading: 'Logical Empiricism (Operationism)' (25, p. 58).

empiricism to the psychologists, it may be well to devote a few words to the question.

Bridgman (4), the number one 'operationist,' has publicly disavowed the term. He takes pains to make clear that operational definition is not an 'ism,' that it is not a philosophic movement or a scientific *Weltanschauung*. It is simply a tool recommended to practicing scientists as an aid to make their meanings clear and as a preventative of unnecessary verbal confusion. Less metaphorically, it is nothing more than a criterion of what constitutes scientific meaning. And least exciting of all, the criterion simply makes explicit the strictures implicitly observed by every meaningful scientific proposition that has ever been formulated. Scientific empiricism has also developed a meaning criterion which is in essential agreement with that of the operationists. But this latter 'school' is more than a meaning criterion. It is an organized revolt against traditional metaphysics and epistemology; it would substitute for traditional philosophy the logical analysis of the language of science. Synthesizing empiricism with the recent work in foundations of mathematics and logistics, it has devised fruitful logical and methodological tools for the purpose of such analysis.

Before we go any further we should note a significant difference in perspective between the meaning criteria of 'operationism' and logical empiricism; one that has rarely been made explicit. Bridgman's formulation approaches the process of definition from the incidence of the scientist's *activity*. A concept finds its meaning ultimately in the specific experimental operations that some scientist performs in some laboratory at some time. The most sophisticated formulations of the meaning criterion of logical positivism approach the problem from the point of view of analysis of the language of science.

Strictly speaking, the determination of the meaning of a given concept is, for operationism, an experimental problem: one reconstructs the meaning of a concept out of its context in the matrix of scientific activity. For scientific empiricism the determination of the meaning of a concept is a problem of applied logic. One refers to sentences, not operations, *i.e.*, to

the end products of scientific activity and not to the activity itself. On this interpretation Bridgman's criterion is relevant to *psychology* of science,[17] while the meaning theory of scientific empiricism is a branch of the *syntax* and *semantics* of the language of science.

Nevertheless the avowed purpose of Bridgman's meaning criterion is not to investigate the psychological conditions of concept formation, but to provide a tool for the clarification of meanings. This is identical with the purpose of the empiricist meaning criterion. Since the sentences of science are the carriers of scientific meanings, any analysis of scientific meaning must take linguistic materials as its subject matter. In identifying meanings with operations then, Bridgman is using an improper mode of speech. It is quite easy to translate Bridgman's criterion into one with a linguistic rather than a psychological reference. As a matter of fact, this is what everyone who has ever used it as a device for the clarification of meanings has been implicitly doing. When such translation is made, we find it to be in essential agreement with the criterion of scientific empiricism. There is one slight difference, however: the empiricist criterion has been much more precisely formulated, and elaborated in greater detail. Thus, substitution of the more rigorous empiricist criterion for the criterion advanced by the operationists appears desirable.

Bridgman's (4) famous dictum that '[the concept is synonymous with the corresponding operations]' may easily be thrown into essential agreement with the meaning criterion of scientific empiricism. Simply substitute for 'corresponding set of operations' the expression: 'the totality of rules describing the corresponding set of operations' and one already arrives at an uncouth first approximation to the empiricist criterion. The rules describing the corresponding set of operations are, of course, our old friends, the reduction sentences. The only liberty that we take in making this identification is to advance an explicit definition of 'operation'—something that Bridgman has failed to do. 'Operations,' on this view, become the specific behavioral acts which an inquiring organ-

[17] Stevens (28, 29) has recognized this by identifying 'operation' with 'discriminatory reaction.'

ism must perform in order to *apply* the 'Q_1,' 'Q_2,' etc., predicates to which a given concept is reducible. The operations which give a concept its meaning are the operations of realizing the experimental situation (Q_1) and observing the result (Q_2). The ultimate elementary operations to which the process of operational definition must lead are described by the observable thing predicates.

We therefore identify the demand for operational definition with Carnap's proposal: "Every synthetic sentence must be confirmable." And the particular form which the operational definition of a concept must take will be the specification of the totality of its reduction sentences.

Adopting the reduction sentence as our technique for the operational definition of scientific concepts enables us to correct a serious difficulty in certain of the loose formulations of the operational principle current in psychology. Pratt, for instance, says: "Exact definitions of psychological concepts are best given by functional equations. On one side of the equations stand experimental observations and 'operations'; on the other, a word or phrase designed to comprehend them" (25). The ambiguity flowing from the fact that no one except a Korzybski patient has ever seen an operation or observation standing on one side of an equation, is easily corrected. More serious, however, is the identification of *all* operational definition with an equation, *i.e.*, with explicit definition. We have taken pains to point out the very fundamental distinction between an explicit definition and the conditional reduction sentence. Disposition concepts are introduced not by an equation but by the conditional reduction sentence. This follows from the fact that the meaning of these concepts is never completely determined—it is always possible to add new and independent criteria. To insist on an equation is to place an obstruction in the path of scientific development: a rather extravagant gesture in the young science of psychology.

The Reduction Sentence and the Instinct Controversy.—A science in the throes of developing a methodological super-ego should proceed warily. The psychological journals of the 20's

were big with a wordy issue sometimes referred to as the instinct controversy. The left wingers of the time—among them Kuo (**20**), Holt (**13**), Faris (**12**), Dunlap (**11**) and Watson (**31**)—were recommending either complete abandonment of the instinct theory or thoroughgoing revision. The attack was conducted on several fronts,[18] but only one is relevant to our present purpose. This was the claim advanced by Allport (**2**), Holt (**13**) and others, that 'instinct' is a concept having the same methodological character as 'faculty' concepts, *i.e.*, 'instinct' is simply a class name for a group of phenomena posing as an explanation for those phenomena. The fallacy of hypostatization was appealed to, and the tendency to reify words into causal agents was decried. To quote Holt (**13**, p. 4):

> In view of the general contempt in which faculty psychology is held, it is remarkable that anybody can fail to perceive that "instinct" as here employed is merely a synonym for faculty. Yet so great is the reifying power of words that even in so flagrant a case as this, the mere name of the phenomenon is accepted by many as the *vera causa.*

With the aid of the reduction sentence, it is quite easy to show that such an objection—intended as it was as a rousing defense of scientific method—involves consequences that would legislate most scientific concepts out of existence.

The objection under consideration implicitly presupposes that the definition of a scientific construct necessarily takes the form of an equation (explicit definition). To take a very simple example, suppose someone asserts, "Pigeons have a homing instinct." Holt and his confreres would immediately point out that the only evidence for this 'instinct' is that pigeons do, as a matter of fact, under certain conditions return to their home-cages. To assert on the basis of this that pigeons have a homing instinct is for Holt only another way of saying that they return to their homes. Ex-

[18] Tolman (**30**) lists five. They are: (1) nonvariability, (2) arbitrary grouping, (3) assumed 'preestablished harmony' between organism and environment, (4) faculty psychology, (5) fundamental instinct-habit identity. While the ocean of obfuscation enveloping instinct theory merits some of these objections, we will attempt to show that (4) is based on a faulty view of scientific method.

plaining the pigeons' homing activities by assuming a 'homing instinct' is tantamount to saying that pigeons fly home because they fly home—something that looks very much like a tautology. Now this criticism [19] could only hold water if (1) the *only* symptom for the homing instinct is the return of the pigeons to their nests, and (2) the instinct is *equated* with its symptom. In brief, the critics must assume that 'homing instinct' is introduced by an explicit definition, containing a single predicate in the definiens.

But we have seen that all motivation constructs have the character of disposition predicates, and as such are introduced by *conditional* reduction sentences, and not explicit definitions. Identifying Q_1 with 'removing pigeon X to such and such a distance from his nest,' Q_2 with 'X returns to nest,' and Q_3 with 'X's behavior as determined by his "homing instinct"' we may write the following bilateral reduction sentence:

$$Q_1 \supset (Q_3 \equiv Q_2).$$

Q_1 and Q_2 are now seen to be only one among an indefinite number of possible classes of predicates which might serve to introduce the concept. But suppose even that Q_1 and Q_2 *are* the only extant predicates which determine the application of the term. It is always possible that new reduction sentences (symptoms) for 'homing instinct' may be added (discovered) in the future. It is this consideration, we will remember, which makes it unwise to identify the extant reduction sentences which define a given concept with its explicit definition.

We have noted that the number of reduction sentences which can be given for a concept is a partial index to its fruitfulness. The fact that we can, at present, write only a few reduction sentences for most instinct concepts, accounts for the tendency to look upon them as suspect. But there is no indication as yet that any of the immediate successors [20] to the concept of instinct in psychology can muster in their behalf a more impressive list of reduction sentences.

[19] The incredible insouciance with which many psychologists and sociologists can postulate *ad hoc* instincts to fit any occasion would seem to justify this criticism. In many cases it does. But this does not permit generalizing the criticism to all instinct concepts.

[20] 'Drive,' 'set,' 'attitude,' etc.

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[MS. received May 1, 1940]

THE BEHAVIORAL SUPERFICE

BY ARTHUR F. BENTLEY

I

The following matter-of-fact question is proposed for matter-of-fact examination and reply:

Where are we to establish the boundaries of *a behavior?* At the skin of the behaving organism? Somewhere within its skin? In some region beyond? Or may different locations be found?

The question is not "*What* are the boundaries of a behavior?" nor "Do behaviors *have* boundaries?" nor "Does such a thing as '*a* behavior' occur in distinctive factual isolation for research?" It assumes the presence of localized and bounded fact. If no boundaries, then no determinable location;[1] if no determinable location, then no behavior as a proper subject for research, and presumably no scientific inquiry whatever.

In fuller statement our assumptions are: (1) that our purpose is scientific; (2)that by science we understand search for fact; (3)that fact, for science, has always the form, *a fact;* (4) that *a fact* must always have determinable location; (5) that all such determination is through observation and description, with a range from early crudity to late refinement and precision.

Accompanying attitudes during work are: that casual speech-forms—'beliefs,' 'truths,' 'realities'—are irrelevant; that equally irrelevant are the philosophical subtleties buzzing around like gnats in a swamp; that we never seek slovenly sanctuary in a claim that our personal 'knowing' is higher grade than our critically achieved scientific 'saying.'

[1] The present status of the electron favors rather than conflicts with the assertion of the text. What the electron has lost is merely an old form of material location of a highly specialized geometrical type. The lost form is one to which no behavior ever attained except in magical ascription (5).

The above attitudes and assumptions are naturalistic. They are techniques of inquiry to be strictly maintained, but beyond that they claim no status.

Our inquiry concerns organisms with differentiated skins which travel and thus have varied behavioral openings. Within this range the present paper gives attention primarily to everyday human behaviors. Any determination, to be worth while, must, however, be capable of extended application across both the 'simpler' animal behaviors and the 'more complex' human behaviors—though no immediate efforts at such extensions are here made.

The word 'behavior' is to be understood as the noun to which the adjectives 'psychological' and 'behavioral' equally apply, so long as they are equally free from both spiritist and mechanist implications. A behavior, naturalistically, will then be a phenomenal process or other fact which, as our knowledge stands today, cannot be described directly and adequately in either physical or physiological terms. Just as the viruses now offer a common meeting ground for chemist and physiologist in the regions lying between the definitely physical and the definitely physiological (**27**, p. 552), so the many varied transition forms we are acquainted with between the clearly physiological and the clearly behavioral have their unquestioned scientific status just as they come, and do not require us to make behaviors technically physiological any more than to make vital facts technically behavioral—or for that matter than to make either or both narrowly 'physical' in technique.

II

Organic 'skin' is a phenomenon variously appraised. Removed and tanned, and thus no longer in process of life-change, it is called leather. Statically and structurally viewed, and so in provisional disregard of life-change, it has status as the 'fact' called 'skin' in ordinary speech and in anatomical inquiry; here it is viewed as a *part* of the organism with the boundaries of the organism established as if by metric scale 'outside' it. The physiologist studying life in process can,

however, not manage with such factual simplification; cells are exploding; chemical, electrical, and radiative changes are under way; component electrons do not tolerate older types of localization; events come to require highly specialized durational descriptions; to the physiologist the skin may readily thus appear less a *part* of the organism than as a region which connects the 'inner' organic and the 'outer' environmental phenomena.

Our initial question may now be sharpened to read: Does the physiological skin, as physiologists depict it, suffice for demarcation of behavioral phenomena, or is some different boundary region required for their adequate description?

The data here to be presented include certain observations psychologists make and certain observations others make upon the progress of psychologists as observers. Our conclusion will be that for many important purposes we must regard *a behavior* as a process, activity, or event that ranges through a region wider than that within a 'skin.' Matter-of-fact observation, to be maintained, must have precise, literal statement accompanying it. If the behavioral event *is* wider than the physiological skin, if it is actually so observed, then *literally* we should report the locus of the behavior to be wider, and so also its *boundaries*.

To simplify discussion, let us hold the word 'skin' strictly to its physiological use, and let us revive the obsolescent word *superfice* (accent on the first syllable)[2] for any possible boundary of a behavior other than at a skin. *Intradermal* will be a convenient adjective applicable to phenomena taken as within boundaries provided by a skin; *transdermal*, though not so precise, may be applied without risk of confusion to behaviors taken as requiring a superfice wider than a skin. The possibility is not excluded that skin and superfice are the same in any, many, or all cases of behavior. Our evidences of differentiation will nevertheless be strong enough to destroy the casual assumption of such a sameness. The case of the physi-

[2] Taine's translator (29, p. 299) employs the word (though differently) in writing "it remains to be shown how . . . the localizing judgment situates certain kinds of sensations beyond our nervous superfices."

ological skin already indicates that rigidly prescribed spatial boundaries will not suffice for adequate report.

III

Our inquiry may proceed most readily by letting the pertinent facts group themselves with respect to three questions subsidiary to the main question: (1) Do existing descriptions of behavior hold themselves faithfully within skin boundaries? (2) What accuracy and coherence in specifying fact is being attained by psychologists who unquestioningly assume intradermal localizations? (3) What trend, if any, towards transdermal localizations is found in the newer psychological constructions?

With regard to the first subsidiary question, suppose we take 'voting,' assert it to be human behavior, and try to give it an intradermal description. Under the assumptions set up as naturalistic, we are concerned not with voting in general, but with the specific case of 'a voting,' and this must come into still closer focus as 'a voting for a designated person (or project) at a stated time and place in a given communal election.' If such 'a voting' rates as 'a behavior' at all, then certainly it is one which cannot be given an intradermal description or location. A full-mirroring Leibniz monad might embosom it; short of this the alternatives seem to be either to recognize it as transdermal or else to deny it status as 'a behavior' altogether. This last procedure is that of many current inquirers (some openly, others in the practical outcome) when they isolate a purported psychic or neural component as intradermal, oppose to it certain other components as physically or socially extradermal, place a mechanistic harness upon this mixed psychic and non-psychic team, and surround the whole with a smoke-screen of vaguely fuzzy wordings. Such a procedure condemns itself. Naturalistically a voting is an event of organism-in-environment; held naturalistically in view, it is 'a behavior,' and as such it is directly transdermal if it is anything at all in process, description, or fact.

However a thousand illustrations such as this would make

no dent upon the man who loves his accustomed social or mental fictions better than his straight observations. I shall be compelled to turn to material of a much less important and compelling character, which by the very fact that it does not make so strong a challenge can more readily be examined for what it is worth as observation in progress. The men now to be cited represent several conflicting viewpoints, and no appeal is made to them in support of the specialized positions of this paper, our interest lying instead in the fact that in the ordinary course of their procedures they broke down in one way or another and at one time or another the conventional barriers to observations, *i.e.*, the skin.

Comment has long been made by psychologists that just as a man locates sensation at the point of skin-contact, so a finger-nail tip, fleshless and nerveless, will enable him to 'feel' an object as at its far end; so also will a lead-pencil or a walking stick. Taine's illustrations cover several pages (**29**, p. 290 ff.). James takes his cue and develops it still further (**13**, vol. ii, pp. 33–43), with the support of Lotze in his capacity as physiologist. Both Taine and James were realistic up to the possibilities of their generations. In adjacent fields we find ethnologists and sociologists alike, discussing tools and clothing as 'extensions' of man's body. Theoretical treatment on this basis is far from common, and most observers would doubtless say they were merely using a figure of speech. M. Bentley, however, in discussing environments as one form of psychological context, has made the express pronouncement that "in our body-coverings, hand-tools and weapons . . . we have 'outside' attachments which might well—save for our *arbitrary delimitation at the rind*—be functionally partitioned with the organism, quite as much as hair, claws and teeth, instead of with the environment" (**6**, italics mine).

In two papers dealing with habit and visual perception, V. C. Aldrich has made a carefully planned development along this line. In one he examines ball-playing as the skill of an 'acquired body' (with ball and playground among this body's members) rather than as the skill of the 'native body' alone (1). In the other he deals with vision as a process in which

light-rays literally 'touch' the object in the same sense that skin, or nail-tip or cane touches it. He concludes that we must take into account "an expanding or expandable ego, incorporating parts of the physical environment into its own body—eventually *being* such a body—and thereby extending the range of its immediate experience" (**2**). He locates 'sensation' at the end of the stick, and 'visual data' at the far terminus of the light-ray, rather than in neural or cortical centers.[3] We have here cases, one for perception and another for habit, in which the investigator, though still employing the old psychic terms, finds it desirable to establish locations for behaviors which exhibit boundaries of a form comparable to that we have labelled 'superfice.'

Consider such a procedure in the light of a common form of report upon the organic nervous system (*e.g.*, Pavlov, **26**, p. 207) to the effect that its functions fall into two divisions: first the integration of the work of all parts of the organism, and second, the connection and equilibration of organism and external conditions. This manner of report, where stressed, is equivalent to separating a narrow physiology of organic living from a broad physiology of behavioral living, or in Pavlov's somewhat prejudicial phrase, a "lower nervous activity" from a "higher." If the reports of Taine, James, and Aldrich are literally correct—and if not literally intended, they should not be made at all—they indicate that while a physiological skin serves as a proper boundary for the 'lower,' or integrating, functions of the organism, a behavioral superfice may be required to demarcate the equilibrating, or so-called 'higher,' functions. To appraise this, one should accept the reports as they come and for what they are worth, without either bluntly rejecting them on account of their strange appearance, or sentimentally over-stressing them as if in any way conclusive.

Let us next consider each particular instance of behavior

[3] Aldrich may be criticized for treating a light-ray in geometrical simplification as if it could be snipped off at a given point; also for his localization of 'sensations' and other 'psychic' data as if determinately within a pre-existent surveyor's space. These lines of criticism are, however, not destructive of his approach, but rather hint at its further development.

as referrable to some source or locus called 'the behaver,' and let us speak of the behaver in much the way that we speak of 'the sun' when we make it comprise light-emissions, sunspots and all other solar activity. Such a behaver involves, or is, the organism. He involves, or is, it naturalistically. But how? Common names for the behaver are 'I,' 'me,' 'ego,' 'soul,' 'mind,' 'psyche,' 'self,' 'person,' 'actor,' or even 'whole organism' in the case in which the word 'whole' becomes a bit of mystic personification as a supplement to organic process proper. We do not speak of a quality of sunspottishness, nor of a solar-spotting 'faculty,' but instead look upon sun and spots alike as localized facts, no matter how margins or durations vary. Naturalistically we shall proceed similarly with the organism, whether with respect to its behaviors or to its status as a behaver.

This is just what William James did in his famous chapter on 'The Consciousness of Self' (**13**). He was strictly the observer and describer, and his achievement, despite its wide echoing, has never yet been adequately utilized. Commenting on the difficulty of drawing a line between one's 'me' and his 'mine,' he put the poser: "Our bodies, are they simply ours, or are they *us?*" He then proceeded: "*In its widest possible sense . . . a man's Self is the sum total of all that he* CAN *call his*—not only his body and his psychic powers, but his clothes and his house, his wife and children, his ancestors and friends, his reputation and works, his lands and horses, and yacht and bank account." He offered a rough classification of the constituents of the Self into the material Self, the social Self, and the spiritual Self, with a possible fourth, the 'pure Ego.' He tossed out all Self in the form of 'substance,' retaining as active only the passing thought in its 'stream'; he reported that the sole observation of such 'activity' he had been able to make was in the form of "some bodily process, for the most part . . . within the head"; he retained as soundly objective materials for his purposes of inquiry only items of description such as those above listed. Most certainly if we seek a superfice for the 'me' or 'Self' of James, we shall find it running far afield from the physiological skin, which vulgarly holds the place.

Ernst Mach was physicist primarily, even though his close analysis of the materials of physical knowledge has made men dub him philosopher. The 'limits' of the 'ego,' he held, are indefinitely displaceable, and may factually shrink and expand; his firmness on this point encouraged Aldrich in the inquiries mentioned above. Mach sought observable facts, and worked with them as closely as the terminological facilities of his day permitted. In the following passage he anticipates the most modern form of expression:"As soon as we have perceived that the supposed unities 'body' and 'ego' are only makeshifts, designed for provisional orientation and for definite practical ends . . . we find ourselves obliged, in many more advanced scientific investigations, to abandon them as insufficient and inappropriate" (**18**, pp. 13–14; *cf.* also p. 3, pp. 357 ff. and **19**, p. 9). Such a sentence might easily serve as text for the entire present discussion.

George Herbert Mead made the observation and description of the behaving organism his life work, agreeing with Mach as to what needed abandonment, but making his own very different progress towards transdermal statement. His results are difficult to appraise because he was professionally enmeshed in philosophy's locutions of 'self,' because the old and the new were always striving for dominance in his expression, and because all his main work, posthumously published, lacked the benefit of his personal revision. But Dewey said of him that he had the rare "power of observing common elements, which are ignored just because they are common" (**22**, p. xxxviii), and his approach was undoubtedly, as one of his editors wrote, 'that of the painstaking, first-hand investigator, viewing the subject in its concrete detail, and allowing it to tell its own story' (**22**, p. xiv). He early described the 'self' as social (**21**, **23**), and the 'social' for him was never the environmental crudity that it is in many of its slippery current uses, but a process overrunning the limitations of skin. "The locus of mind," he found, "is not in the individual" (**24**, p. 372). "Our primary difficulty . . . lies in our tendency to cut off life and consciousness at the boundaries of the organism" (**22**, p. 72). Here are no philosophical generalities, but

his directly psychological report, which even his closest followers fall short of attaining, when, stressing mediate acts rather than primary, they see him placing what they call the *terminus a quo* of responses at "the bounding surface of the organism itself . . . from which all responses proceed" (**24**, p. xxxii). Philosophically he may be represented by his assertion that "it is illegitimate to place the original act within the organic individual as an object" (**24**, p. 147). In social-mental emergence "the interpretation of gestures is not, basically, a process going on in a mind; . . . it is an external, overt, physical, or physiological process going on in the actual field of social experience" (**23**, p. 79). Without any hesitation I characterize this work of Mead's as a hard-won report on facts, and all the more strongly as his own followers so often backslide in the midst of their admiring praise.

A further illustration may be lifted from the latest issue of a psychological publication. In contrast with what may seem to be generalities in some of the preceding citations, this illustration rises directly from daily laboratory practice. Myrtle B. McGraw, summarizing the methodological implications of research into infant behavior, reports with literal intention that "in studying the development of reaching-prehensile behavior of the infant, the object in the field of vision is just as much an integral part in the organization of the behavior as are the arms, fingers, and eyes of the baby" (**20**, p. 84).[4]

[4] Casual phrasings of this type turn up frequently, and often where one would least expect them. Thus David Katz in his recent *Animals and Men* (**15**, p. 131) discusses at length phenomena described as "the spatial 'here' which belongs to me as the shell to the snail." Of very considerable significance is the report upon the use of scientific instruments made by V. F. Lenzen in his appraisal of empirical science in the *International Encyclopedia of Unified Science* (**16**, pp. 28–31). With much illustration Lenzen shows how instruments function in frequent alternation, sometimes as phases of the observing scientist, and at other times as phases of the observed phenomena. He carries his report to the physical indeterminacy of momentum and position, where the issues involved have undergone so much confused epistemological discussion that I have deliberately eliminated all reference to them from this paper, despite their probably critical importance for ultimate decision. The matter-of-fact way in which Lenzen has set down the pertinent facts leads me, however, to insert this reference here; more especially because his point of view lies so far over on the 'atomistic' and 'mentalistic' side that no one can possibly suspect him of a bias in favor of any such interpretation as I have been advancing.

The above data make no pretense at 'deciding' anything, but the evidence they offer is sufficient answer to our first subsidiary question. Competent descriptions of behavior do *not* hold themselves faithfully within skin boundaries where efforts are made to secure direct detailed observations in modern forms of insistent objectivity.

IV

We proceed now to our second subsidiary question: When an intradermal location is unquestioningly assumed, what degree of accuracy and coherence do psychologists succeed in attaining in their reports on facts?

The terminology of environments as used in psychological inquiry should provide us precisely the test we need. Organisms have environments. If the behaver *is* the organism, then the organic environment *is* the behavioral environment. If the behaver is a 'part' of the organism, the question becomes more complex. The same is true if 'a behavior' can be investigated as itself environed. In an older age the behaver presented itself as fellow-traveler in a way that made the organism itself environment to it. Soul had World, Flesh, Devil, and God too perhaps, as environments. Psyche had body as well as outer world environing it. Neither Soul nor Psyche ever acquired technically coherent description in these respects; hence both disappeared from modern forms of research. Our present-day naturalistic specifications for the behaver must expect to abide by comparable tests in the long run.

We may properly start by examining biological environments as ecologists see them in their primary forms. Allee and Park have made extensive study of what they call 'ecological principles,' *i.e.*, of the terms and phrases used by ecologists to control their reports upon fact (**3**). They specify the organism as "living," and the environment as "everything in the universe external to the cells and intimately utilized cell-products of that particular organism." They concentrate upon "phases of direct importance," name these the "effective environment," and make the distinction quite properly one of

degree. They elaborate extensively as regards organism-environment, organism-community, and community-environment, and their results here are well worth appraisal with respect to the forever-open issues of the situationally systemic *vs.* the mechanistically interactional forms of report. Here we must limit ourselves to inspecting the words 'intimately utilized cell-products' in the specification of environment. We recall at once what James and others have said about such cell-products, and also about tools and clothes. We find that Allee and Park in the very act of establishing the 'living' organism on the one side and the environmental 'everything else' on the other, exclude these cell-products from the reckoning altogether. In their most basic 'principle' they introduce us to A, B, and C, proclaim the banns for A and B, and leave C a bastard outcast. We readily admit that in their special inquiry no harm results; they have fair right to expect that C will take care of itself when the time comes. But the case is entirely different when one advances from ecological to behavioral inquiry. Here Allee and Park's distinctions are plainly inadequate.

Consider now the verbalism: 'internal environment.' Let us ask what it is that an internal environment environs, behaviorally speaking, and with attention, of course, always directed to the specific instance. Agreement should be possible, one may hope, that no environment exists to itself alone, and that without a 'what' that is environed the word 'environment' simply does not make sense. Paraphrasing a line of Browning's "environments environing with naught they environ" are bad fantasy, let alone science. We all know what is referred to by 'internal environment'—something like "as the thyroid's bulged, the child's inclined." But merely to know this is not enough, since one great difference between science and folklore lies in insistence on the coherence of report. Claude Bernard stressed the *milieu intérieur* by which he meant blood as environment to cells. He made it the basis of general physiology. With him entered the 'intra-organismal environments' (in Allee and Park's terminology) which Haldane, Henderson, Cannon, and others have devel-

oped with admirable physiological results (**25**). But a world of difference lies between physiological description and the way the 'internal environment' commonly enters psychology. A symposium, deeply satisfactory to its participants, on the general subject 'The Internal Environment and Behavior' was held at the 1939 meeting of the psychological section of the American Association for the Advancement of Science (**8**). The discussions had to do with oxygen, water balance, blood sugars, and internal secretions. The pattern of the papers was that of "broken legs and behavior" or of "the presence of water in relation to terrestrial life." They fall within the scope of psychology where 'psychology' is a blanket term for an occupational group, but not where it signifies a form of scientific inquiry distinguishable from physiology through its specialized attention to the behavioral adjustment of organism and environment. When the falling cocoanut hits the man on the head he acts 'queer' for a time, but neither dropping cocoanut nor queer-acting is technically behavioral process.

Seeking an answer to our question as to what the internal environment environs, we first inspect the 'external environment.' Passing over the dermally marginal cases, we note that for primary biological purposes the external environment unambiguously environs the 'organism.'

We next inspect 'intra-organismal environments' in the strict physiological case, and again have unambiguous presentation, whether the word 'internal' happens to be used or not. Within the organism certain regions of function are definitely examined as environing others.

Passing to the behavioral case we have, by contrast, ambiguity and little else. Of the many possibilities for the status of the 'what' that is internally environed, we may select as typical:

1. The case in which a psyche, soul, mind, or self is frankly retained as the behaver, the environing thus being of the form, body to mind. This is of the older, non-naturalistic type which is wholly outside our present range.[5]

[5] I have said nothing in this paper of the magic powers and essences and of their survivals, since in a preceding paper (**5**) I have extensively examined them as to their status in modern science.

2. The case in which despite its formal disavowal such a psyche enters in camouflage as cortex, cortical region, gland, gene or artery. Of this case it should be said that it involves a piecemeal reduction of the psychological to the physiological, regardless of the differences in description and technique between the two forms of research. The attitudes it involves are that such phenomena as horse-seen, word-understood, and plan-pursued are localized physiological events; that ultimately they can be so dealt with; and that the ultimate hope relieves one of the need for present labor. In contrast I take it for granted here that both the authority of physiology in its own field and its technical distinction from behavioral inquiry are properly recognized (**5**).

3. The case in which superstitions such as the sub-mental Freudian homunculi are toyed with.

4. The case in which 'person,' 'personality,' or 'whole organism,' after legitimate observation and graphic description is by sudden prestidigitation transformed into something or other more than the organism itself, thus assuming rank as the IT-proper, to which the biological organism as a humble bodily 'it' can serve as environment.

It is hard to phrase any of these possibilities in respectful language. Honorably the only thing the 'internal' can environ (again omitting marginally dermal cases) would seem to be the 'external' which in turn environs it.[6]

The disregard of linguistic integrity in the case of 'internal environment' arises directly from the unquestioning acceptance of an intradermal behaver as concretely existent. This acceptance is a surviving instance of the attribution of force to a locus in a fashion which has wholly disappeared from physics, and almost from physiology. Its maintenance in

[6] For illustration of an older procedure in which an 'internal' is put forward that can be environed, Hobhouse may be cited. He made sense excitation give rise to a reaction, and this to a 'feeling' as a separate existence, and this in turn to motor effect (**10**, p. 116). A recent illustration of comparable nature is offered by Stevens (**28**, p. 95) who proclaims 'discrimination' as an operationally established psychological power which is at the same time the actual guarantor of all operational science, and who has no hesitation in saying that "by discrimination is meant the concrete, 'physical' reactions of the organism to either internal or external environmental conditions."

psychology, unless it can achieve honesty of statement, will hardly continue long.

If an 'internal environment' is roguery, a 'social environment' is high treason to integrity of speech, though it has indeed something of an historical excuse for its presence, arising from the useful service it performed in the earlier days of sociological fact-gathering. Let us continue to assume that we wish to talk accurately about accurately located facts. We at once face the question as to where 'individual' and 'social' are located, and we are further entangled in an issue as to how a 'social' can be both organic and environmental at one and the same time. We find that it makes sense to say that one organism has another or others for its environment. It also makes sense to speak of organisms in collections, as in rough namings and statistical reports; also to speak of them in complex form, as in the case of a biological species threaded on its genes. But in technically behavioral inquiry—inquiry into adjustments of organisms in environments—it does not make sense at all to talk of 'social environment' when accuracy in observation and report is highly valued. It is chaotic to make the behaving organism both *be* social and *have* the social as its environment. If the behaver is seen as intradermal, and the social as extradermal, how can the former 'be' the latter? If the behaver 'is' social and at the same time intradermal, then the social must be intradermal too and hence cannot be environment. The more one tries to make these bits of wording cohere, the worse they fly apart. This is not a quibble about words, nor a dogma about reality. It is a matter of simple everyday honesty in trying to see straight and tell straight what one sees. If, however, one abandons the intradermal location, and inspects behaver and behaviors alike as transdermal in the way so many powerful observers have felt to be necessary, then as the conflicts of extradermal and intradermal disappear, so also disappear the conflicts of individual and social and those that arise from the harsh severance of organism from environment.

The answer to our second subsidiary question is that a chaos of assertion results where observation of behaviors is

attempted in a non-spiritist way, and at the same time report thereon is made under the influence, explicit or implicit, of the ancient belief in their intradermal location. This chaos is so serious as to indicate the presence of a radical defect in observation, or report, or in both. The evidence is circumstantial, but it nevertheless strongly supports every project for experimentation on a changed basis.

V

The third subsidiary question to be asked is: What efforts at theoretical reformulation are investigators making by way of remedy? Since reference can be made to recent exhibits in this field (**4**, **5**) the briefest of statements will suffice here.

Pavlov, one of the world's greatest experimental physiologists, found himself in the course of his experiments passing from stimulations in the deeper lying parts of the alimentary canal to buccal stimulations, and from these to visual stimulation by distant objects. His great work on 'conditioning' resulted as an express expansion of physiology into new fields. Our interest in it is that while it was solidly based on organism-environment, in its technical procedure it eliminated the older form of behavior-environment. Definitely, not in official statement but in actual experiment and report, the physiological skin here abdicated as the critical dividing line within knowledge.

Kantor some twenty years ago remarked that while probably every one of his colleagues would agree that an organism unenvironed in sense-process becomes behaviorally non-active and that without confrontation of organism and object no behavioral process could occur, theoretical development on this basis had never been attempted. If these facts are true, he argued, they are fundamental for all psychological construction. He proceeded to posit that both organism and object participate in behavior, that their participations are of equal importance, and that behavior is to be regarded as their interaction. While he treated behavioral phenomena as outgrowths of an evolution in rigidly pre-existent space and time, and while this increased the difficulty of his more complex in-

vestigations, his achievement is steadily gaining in recognition.

Lewin has followed a similar path, working in terms of person and environment, instead of organism and object, and organizing them in what he calls a 'field,' in which he portrays them pictorially by devices derived from topology. His work is much less searching than that which preceded it, but perhaps because of this very characteristic, combined with favoring influences from the newer physics and mathematics, it has attracted much attention.

More recently J. F. Brown, possessing a much wider factual background, particularly in sociological research, has strengthened this last-mentioned construction. He examines a 'vector field' in race and group setting, and gives indication of much useful development.

VI

We have found: that matter-of-fact descriptions run across skins; that intradermal localizations lead to terminological chaos; that psychologists are experimenting with the combination of organism and object in basic behavioral statement.

These experiences of psychologists in their pursuit of a science are our data. They answer to Warren's definition of data as "the group of known or 'given' facts upon which a scientific discussion is based." They 'prove' nothing by themselves, but when considered as a group they establish ample basis for hypothesis in aid of further inquiry. If behavioral environing differs from other biological environing in highly specialized characteristics, and if an inquirer faces a problem towards the solution of which a transdermal localization seems likely to offer aid, then the facts here assembled justify his experiment. Emphatic is the requirement, however, that he proceed under the rule of 'all or nothing.' Little ventures into a transdermal, broken by timorous withdrawals into an intradermal, will be as destructive of the newer viewpoint as they are of the older.

Since we have so heavily stressed the chaos of inner localizations we may expect a sharp—and entirely proper—demand to exhibit an adequate coherence in our preferred transdermal

form. Such coherence cannot be delivered to order. It is a slow, painful acquisition. The case against the interior localization of behavior does not lie in any assembly of arguments such as the present paper offers. It lies in 2500 years of failure to get solidly usable results. It lies in the fact that the most powerful efforts at precision—the Cartesian and Leibnizian, for instance—result only in new imprecisions.

Consider the words 'stimulus' and 'reaction' under their commonly implied hypotheses. These words are standard for psychologists, if any are. But who would dare say they were 'standardized' for the science? Only a dashing way with words—only a Cavalier Psychology—could pretend it. *Physiologically* the two terms are dependable. *Behaviorally* no hearer is ever quite sure what any speaker implies. 'Object' does duty at times for stimulus—but 'object' may mean anything. 'Response' substitutes for reaction where one wants to flatter the 'whole' in the organism.[7] 'Symbol' tries its hand at a pinch, sometimes with, sometimes without efforts to sharpen its technical application (**11, 30**). 'Inner pictures,' reminiscent of the kind some savages refuse to have torn out of them by the camera, demand to be coupled with 'outer object.' Percepts, concepts, objects, are inside, outside, or sitting on the fence, part time, whole time, separately or all together—one never is sure.

The manifest of any science should lie in the straightness of its speech. Dewey has remarked somewhere that psychologists today use the terminology of stimulation as crudely and unreflectingly as physicists used that of causation when they first plucked it from the vulgar soil. Forty years ago in a famous paper (**9**) he showed that the components of the 'reflex arc' of his contemporary discussion ought to be viewed "not as separate . . . entities . . ., but as divisions of labor,

[7] I take it for granted that it will be recognized that my repeatedly expressed objection to 'whole organism' is solely to a personification in the manner of the old psychic. Our present argument, being radically opposed to all atomistic formulations, not only assumes the fully functioning organism, but goes still further since it assumes the full organic situation within which the organism is nuclear. Inquiries into person and personality may well be more efficient under this viewpoint than they are when the person is regarded as independent or quasi-independent; they are more fully factual, and in better balance.

functioning factors, within the single concrete whole"; and that an inquirer who pried out stimulus and response separately had nothing but "minor acts" left in his hands. Despite all the discussion it received, this thesis of Dewey's has not yet impressed itself deeply enough upon psychology for it to reorganize its terminology. Independent actors and factors coupled by crude causation appear, and leave the full functional process of behavioral adjustment ignored. Stimulus and reaction, crudely labelled, require the introduction by sleight-of-hand of that fancy fellow-traveler we have already met, to perform magically the work which the language of description and naming itself fails properly to perform.

What happens now—what may be expected to happen—when the full stimulating-reacting event is presented within a superfice, with no physiological skin licensed to cut it into incorrigibly opposed parts? Here the Dewey thesis applies simply and directly. If one keeps results of Coghill and Lashley in mind, it applies on an up-to-date biological basis. What every psychologist knows is that stimulatings and respondings vary selectively in terms of each other. What a transdermal description does is to permit direct technical statement and inquiry on this basis. Behavior is not matched against the organic phase alone. It is physiological, but broadly within a superfice, not narrowly within a skin. It exhibits genuine *phases*, not mechanical combinations. Its namings must be compelled to say this.

In simple illustration the status of superfice, and the need of a direct language, neither physical, physiological, psychic, nor pseudo-psychic, to report the events it demarcates, can be shown. Take walking. Consider it simply, directly, literally. Walking lies in the borders between the physiological and behavioral, as the viruses (**26**) lie in the borders between physical and physiological.

The assertion "the dog walks" is impressionistically usable, but incomplete. Walking requires a footing. Technically it is no more correct to make the dog *do* the walking than to let the land do it. For physical display of walking Newton's third law is sufficient guide. In physiological in-

quiry attention concentrates on the organism, but apart from implicit inclusion of the environmental background no physiologically phrased report could make sense. Evolutional interpretation exalts no legs without land, no wings without air, no fins without water. The coupled participants enter together; in them together the trends must be sought. In the transitional behavioral case of walking, the physiological language plus psychic or pseudo-psychic phraseology to cover stone-avoidance, corner-turning, shadow-sign effects, memory-effects, etc., makes out fairly well. Pavlov fined any assistant who used a 'psychic' term in his reports, but Pavlov was exceptional in his standards of precision.

If we pass to a more intricate behavior—say a runner in an Olympic race, with all the involved 'social' pasts, presents, and futures—the physical and physiological terminologies, basic as they are, do not pretend to reach adequately complete description, while the psychic and pseudo-psychic and all other intradermal localizations are chaotic in their reports.

It is here that we find ourselves required to take the environmental and situational settings immediately and functionally into our descriptions of events instead of dealing with them by special technical device or general presumption as in the physical and physiological cases, or by inadequate verbal makeshift as in the older psychological approaches.

To escape chaos we have, it seems, only the alternatives: either to go completely spiritist, which is, colloquially, to let George (the spirit) do it, which is again to eschew inquiry and negate knowing; or, on the other hand, to take the full event directly and immediately into account—the full event which here, for convenience of report, has been discussed in terms of a demarcating superfice.

Superfice is not marked out with ruler and pencil in geometrical form. This form yields at the best a preliminary approximation to a full behavioral description, somewhat in the way that, though not so well as, anatomy yields an approximation to physiological description.

Superfice is the delimitation of the region of observably active participants in the behavioral event. The behavior

must be marked off in time as well as in space, and the superfice does both. There is nothing mystic or magic about it. In the typical Pavlov case, for example, seen-food is the region —not food alone, nor seeing alone, but the full process: seen-food.

Superfice is a factual specification. When you need it, and if you need it badly enough, you set it up and begin to use it. Euclidean space rose in knowledge the same way, and its validity is of the same order—it is good as far as it works and no further.[8]

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[8] During the preparation of this paper I had entirely lost sight of Walter S. Hunter's presidential address before the American Psychological Society in 1931 (12), with which I must certainly have been earlier acquainted. Hunter contrasts the subcutaneous with the superficial or supercutaneous, leaving no intervening perch for any organism-as-a-whole as a distinctive behavioral participant. His line of 'naturalism' direct from Darwin is that which I follow. My manner of requiring specific observation is closely akin to his. While the differences in treatment are as marked as these similarities, it is clear that I should make note of the extent to which my paper follows lines collateral to his, and also of the probability that I have been influenced by him in one of those indirect ways which, so often, when we forget, we believe not to have existed.

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[MS. received April 25, 1940]

COORDINATES OF PSYCHOLOGY: ANALYTIC METHODS APPLIED TO THE FORMS OF PSYCHOLOGICAL THEORY

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I. Introduction

It is generally recognized that the coordinates of analytic geometry have no material effect upon the analysis of concrete phenomena such as our perceptions of spatial relations. They serve to frame clearly and unambiguously the data of qualitative or pure geometry, and tend thus to be overexact in proportion as quantitative inaccuracies are present. If a geometer were to set forth any empirical problem exactly as he found it, his coordinates would be unaffected though possibly obscured; the essential analytic difference between abstract and real consists in the fact that whatever is real is less precisely determinable. This does not mean that determinants may not be found—it indicates rather that they apply equally well to any specific formulation within a certain region of uncertainty. The surveyor who has plotted a closed traverse calls his discrepancy on paper, *i.e.*, in abstraction, an 'error of closure.' If it is too prominent in the results he runs a more precise survey. We may infer, then, that the real outlines of the plot are determined within limits set by the accuracy of the closure. The probable boundaries are finally established by fixing coordinates for certain actually located 'points of reference,' with other points interpolated within the degree of precision obtained.

It would remove a major source of misunderstanding in psychology if we could by a logical process of analysis discover general criteria or coordinates for the setting forth and clarification of psychological problems, in terms of which we might then state psychological errors of closure, *e.g.*, degree

of accuracy of instrumental aids, or differences between individual procedures. This paper is an attempt to outline such a general analytical frame of reference, to express in non-valuative terms some of the prominent reference points or presuppositions of present-day psychological thinking, and to show how such a general analysis may be applied in those terms to psychological theory.

For convenience of reference, I shall indicate the basic assumptions (*A*) and definitions (*D*) with which I am working, for they represent my own view of psychology, and not necessarily that of others, though I hope that eventually some set of assumptions may obtain relatively widespread acceptance. In addition, I plan to discuss psychological matters in terms of certain points of reference (*P*) and dimensions of theoretical variation (*C*). The following tabulation will furnish a shorthand basis for reference to these various primitive notions. It is not possible or advisable, I think, to give precise and final definitions at this stage, so I shall merely give names and short characterizations.

Assumptions.—The subject matter of psychology can be conceived of as concerned with 'living phenomena' (A_1, the general universe of discourse of psychology), with 'experience' (A_2, the specific universe of discourse), and with 'options' (A_3, the specific subject matter). I add the pragmatic assumption (A_4) that psychology exists as a separate discipline, and not as a subsidiary aspect of another field.

Definitions.—Assumption A_3 may be clarified by reference to the definitions of 'psychology' (D_1, sec. III) and 'psychological data' (D_2, sec. IV).

Points of reference.—Treatment of subject matter will involve reference to 'function' (P_1) and 'the individual' (P_2). The former places emphasis on the study of psychological phenomena in terms of dynamics, the latter upon establishing a focus of attention, to which all analysis may be referred—thus assumptions A_1 to A_3 all involve reference to individuals. In the analysis of theory two further means of reference are useful, to 'purpose' (P_3), as a basis for the characterization of descriptive devices, and to 'method' (P_4), *e.g.*, of investiga-

tion. Together, P_3 and P_4 furnish cross-references for determining the overlap or relevance one to the other of theories which have a determinable universe of discourse or range of application.

Given these assumptions, definitions, and points of reference it is now possible to consider an approach to theory in more general terms, by the use of coordinates of analysis which have no direct relevance to the methods and subject matter of psychology as such.

General coordinates for the analysis of theory.—The more general features of my analysis have been indicated by the section headings. Thus, the four major coordinates which I now recognize and discuss are: 'abstraction' (C_1), 'scope' (C_2), 'data' (C_3), and 'generalization' (C_4). *Cf.* sections II–V. The present treatment emphasizes the limitations to the use of abstraction for description, the variation of scope of treatment and the importance of its recognition, the range of data which may be comprehended, and the uses of generalization in theory. Section VI comprises a resynthesis in terms of the psychological subject matter and the general analysis.

II. Abstraction

"John Smith is born, he lives and he dies." As psychologists we are primarily concerned with his living (A_1). But so too are physicians, biologists, and neurologists; in fact, a very great part of human interest is directed at living phenomena. Say, then, that our concern as psychologists is with that realm of discourse common to intuitionists such as historians, authors and sociologists. Strict psychologists, however, tend not to deal with speculative abstractions (C_1), preferring to leave that task entirely to the philosophically minded. This division of labor is comparatively recent, dating from the first psychological raids upon philosophical armchairs, which resulted in a monopolizing by psychologists of the task of treating man as personality. But this revivifying of individuality brought about the loss, together with that of abstraction, of another habit of the philosophers: critical *a priori* thought in the interests of coherent investigation. In-

stead, there now exists a tendency towards the uncritical and wholesale adoption of methodological and experimental *modi operandi* and theory from physical science. As a result of this neglect of philosophical considerations, it becomes possible to abstract falsely, or to imitate unsoundly.

1. *Uncritical abstraction.*—The law of mechanistic causation may be applied to everything. It results in precise and intelligible explanations, neat formulations without bothersome loose ends of unassimilated phenomena. This sort of explanation is an abstraction—it separates and selects its facts, thus running up against philosophical considerations as to adequacy of representation and accuracy of explanation. In physics the data treated are clearly defined by its methods and instruments; in pure abstraction, *e.g.*, logic or geometry, there is no confusion arising from the necessity for describing things completely—they are assumed to be such and so, and that is all. But in psychology, there is as yet no generally accepted method for selecting basic postulational modes of explanation. In consequence, any abstraction that has had success in another field is likely to be taken over bodily into psychology, and assumed to be useful and exhaustive. Attempts to adopt the law of mechanistic causation outright from physical theory reflect just this assumption.

2. *Uncritical imitation.*—The situation is similar if we seek to obtain the quantitative exactitude and mathematically sophisticated description of physics or chemistry. Our subject, John Smith, is a man of average build. His neighbor, Jones, is rather more than stout. Jones has specially reinforced furniture built for him. Does Smith follow suit? Not if he is sensible. Similarly for the psychologist who is adopting structural or material descripts from another field: His safest course of action is to act consistently or compatibly with the system of purposes centered about the goal or goals in which he is most interested. Consider Jones again. What he did was to choose a chair adequate to support him in security and comfort; such a chair had certain proportions and magnitude. Smith may imitate him in two ways: by choosing 'a chair adequate to support him in security and

comfort,' or by duplicating Jones' chair in every detail. Perhaps, now, Smith's chair will turn out to have the identical proportions or pattern that Jones' has. From that fact alone we cannot deduce anything. The important fact to consider is whether he obtained it by following Jones' purpose or merely imitated his procedure. Similarly in psychology it is essential to know the manner in which new procedures are taken over from other disciplines.

With these examples to guide us, we may turn with some degree of safety to any branch of human endeavor, adapting its principles to problems in the field of psychology. Let me cite as illustration the mind-body problem. In common descriptive terms we say that John Smith has a mind and a body, each having a distinct function: He thinks with his mind, and walks with his body. But he may also have feelings: He becomes depressed, has a headache, enjoys a play. None of these feelings are easily confined merely to the mental or bodily. However, it is of small account. My fundamental assumption (A_1) is: John Smith lives—he is a living organism, an autodynamic entity. That means he has active relations to his world, relations which lose their meaning for me unless considered relevantly to his individual nature and experience. Seen as an aspect of abstraction this concrete view of Smith that I am assuming is unintelligible. For abstraction would require the use of hypothetical structural divisions such as mind and body. And we should probably begin by trying to prove that John Smith exists! Such an approach may be proper to metaphysics or theology, but as a psychologist I feel bound to depend instead upon a direct apprehension of our subject-matter, and to deal with it within dynamic categories. It is not enough to study the structure of Smith's personality; we must discover how he functions (P_1)—what the things are he does, and why he does them.

Personal experience is the basis for critical inference (A_2). The nature of Smith's experience and the degree to which he has assimilated it determine how critical he is, but his interests determine what he will choose as an initial point of reference. Note, for example, where the metaphysician begins: within

himself, in pure abstraction. And where I begin as a psychologist: with the relations of the individual (P_2) to other things. The physical scientist relies no less upon his experience: he assumes virtual stability of human functioning, and goes on from that point to study the general relations of things. These 'general relations' are factually grounded, and are based upon the cooperation of many men. Thus we do not round the circle, for the general relations of the metaphysician are not grounded in facts or verifiable through collaboration.

III. The Scope of Psychology

Thus far I have mentioned and dealt more or less simply with some of the general features of psychology, but without yet defining its scope (C_2). To do so completely is virtually impossible; in a great degree any definition is superfluous; but there remains some divergence as to terms and things dealt with. Realizing all this, I venture to mention synoptically what I conceive to constitute the essentially agreed upon facts [1] of psychology. That it has to do with living beings (A_1, P_2), notably in their relations to activities of various sorts, will scarcely be denied. John Smith eats, rests and occupies himself. So does an earthworm. Yet Smith's life is certainly different from the worm's. Different in form, richness, dependence upon structure—in all modes of functioning. In short, their schemes of life differ greatly. But, it seems to me that they share certain primary functions, in terms of which (P_1) we may hope to describe the complete situation. To this end I seek now a means of distinguishing between any individual's psychological makeup or functionings and his primarily biological aspects.

Let me repeat and rephrase a statement already made. Mechanistic explanation does not suffice to account for the activities of living beings if the complete situation is to be

[1] A 'fact' includes what is given to experience, and some sort of interpretation, which may be conscious or unconscious, emotionally grounded or not. When two people come to interpret alike, *i.e.*, when they hold theories that are formally identical about what they both feel is the same subject matter, their facts are the same. This statement will be refined and amplified in a later article.

taken into account. That which lives has the property of delaying, slowing down and canalizing the otherwise simple dissipation or transmission of energy. A stone falling off a bench remains where it fell. An egg near it falls too: Out walks a chick which lives, distributing energy and constantly altering the arrangement of things while it lives. If psychologists assume life, they cannot help but deal with these latter phenomena. But the question remains, in what manner?

Suppose I say (D_1) that *psychology* is a mapping out and systematic consideration of *the optional features which exist or may be induced in the relations of living beings to the conditions of their life.* Even the earthworm exercises some option (A_3) with regard to his path through the soil So, too, Smith may choose to eat oatmeal for psychological reasons, *e.g.*, its taste, or the influence of advertisements. If he were on a desert island with only bananas for food, bananas in their relation of food would not have this free choice for him.

Non-options are those functions of living which the individual is unable to modify. In the broadest sense it is evident that he may not choose a different organic existence; he may rearrange his surroundings within certain limits; what he perceives and remembers is somewhat more under his control; his conscious activities may be most nearly viewed as freely chosen. In short, so far as Smith is dependent upon the conditions of his life, his behavior is non-optional.

Certain non-options have psychological significance, *e.g.*, the results of childhood training or other past experience. However, many others have no such direct relation to individual experience. It is part of an individual's training that he explores these limitations, and to that extent they have psychological significance. Aside from this incidental overlapping of other disciplines with psychology, there is a legitimate field of inquiry in boundary line matters. For present purposes I assume (A_4) that psychologists are adequate to the task of mapping the boundaries of their field; however, in my opinion, the central task in psychology is the mapping out and study of the possibilities of option in individuals.

The argument to this point has been general in scope (C_2). Within the bounds indicated, however, it is quite allowable—even necessary—for psychologists to limit themselves somehow both as to the material and the relations studied. For example, I might be concerned with the personality of John Smith, average American adult, member of the herd, quasi-leader, a man of fixed habits in a changing world. Or with his emotions, his marriage, his leisure. Or specialize on learning: in rats, children, apes. In doing any of these I could generalize from my specialty and claim that my specific preoccupation *is* psychology, that its rules and concepts comprehend the entire scope of the field. Doubtless I should make progress; but it would be in a circle. I prefer, instead, to outline the task of integrating and coordinating the work of others in the hope of arriving at a statement of certain major psychological issues in terms of which all psychologists might be willing to express themselves. Accordingly, I plan first to state definitely, if somewhat prescriptively, the nature of psychological data (D_2), then to consider how they may be reduced and simplified (C_4), turning finally from description (in terms of reference points P_3 and P_4) to inference and application (as applied to formulations of theory).

IV. The Data of Psychology

Anything that happens to John Smith is capable of psychological consideration. For, living as he does (A_1), he can always choose or find options (by definition). And, living as we are, we may consider his non-optional relations (*cf.*, A_4). What, then, are the data (C_3) of psychology? A lamp *per se* is not. But considered by me it is. *Per se*, *Alice in Wonderland* is not a datum in psychology. But as written by Carroll, or read now and remembered later by Smith, it is. When Smith was five he fell in the water. Now he dislikes bridges. Which is the datum: the bridge he fell off or the water? Neither of them, strictly speaking, but rather the fall which involved them both. This fall affected Smith organically, but he 'got over' it eventually. His present dislike of bridges is a psychological datum or functional process.

Ultimately it may be advisable to undertake a comprehensive treatment of the phenomena ordinarily included within the subject-matter of psychology: learning, emotion, habits, the psychoses. But for the present let me formulate a general criterion (D_2) for *psychological data:* they are *instances of those things which exhibit such functional relations as to affect the living organism or individual* (*e.g.*, Smith) *in his possibilities of choice—in his options.*

V. Generalizations in Psychology

I have employed generalizations (C_4) before this stage. But they have been methodological, analytic, or indicative, with interpretation excluded as far as possible. What I now propose to discuss under the heading generalization are those abstract terms introduced into psychology for the purpose of simplifying and organizing data. If we except prevision and intuition, which are synoptic short-cuts, it is necessary that the data dealt with be already given before we introduce generalizations to deal with them. Unless this possibility is taken into account the abstractions used may appreciably alter the nature of the data to which they are applied, since things tend to be seen in the light of what is already known or supposed about them rather than considered in and for their real appearances. Even the words used to designate generalizations may cause confusion, for any word has a plurality of meanings. And the possibilities of running astray are greater still if the term is part of a system that someone seeks to impose universally. In fact, to say that a given generalization will apply universally, *i.e.*, as a law, is either to phrase a logical generality or to speak nonsense. I hope to avoid these difficulties of interpretation by considering generalizations with regard to their utility in dealing with specific purposes by definite procedures—a mode of development which precludes subsidiating generalizations to any universal and explicit non-analytic system.

In this sketch I cannot deal with every general term employed in psychology, but only with a few that concern some of its major purposes and methods of investigation. I

shall construct a cross-graph such that we can locate within limits any given generalization in terms of purpose and method (P_3 and P_4). But first it may be instructive to name some general terms and trace their sources.

Description	Inference
everyday speech	*theory*
habit	conditioned reflex
memory	trace
purpose	goal-seeking
instinct	propensity
intelligence	'g' and 's,' I.Q.
mind	ego
concept	operation
	application
the unconscious	unconscious
feeling	cathexis
forgetting	repression

It is evident from the first list in the scheme that 'descriptive' terms apply to a great part of psychological endeavor—evident moreover that they simply coexist and are not well related or systematized. Further, it seems that their interpretation depends more upon categorical or normative considerations than upon data. On the other hand we find corresponding terms which—ideally at least—represent *a posteriori* attempts to find general principles of interpretation based directly upon data; this 'inferential' mode of attack sets the stage for analysis of data. If we are content merely to point out uniformities in data, descriptive generalizations are quite adequate. But the ordering of those uniformities requires greater preciseness: Thus we have 'field,' 'propensity,' and 'configuration' as terms in general systems, and the terms 'equilibrium' and 'repression' which display special or applied emphases.

Roughly speaking, the data of psychology may be considered in the light of four major types of investigative *method:* collection of selected facts, intuitive categorization of data, experimental observation and analysis of data into functions, qualities and relations, and clinical or practical dealings with the person as an individual. If we plot them against the two types of *purpose* I have already noted—description of data, and inference from data—we have a frame for characterizing

general terms. I include analogy with misgiving, and only because of its misuses; confining its use to illustration is a first step toward analytic integration of psychological data.

Method	Purpose	
	Description	Inference
Analogy.....	(anecdote)	(borrowing from other fields)
Classification.....	hysteria	dissociation
Experiment.....	intelligence	'g' and 's'
Application.....	the unconscious	unconscious

If I had given more than one term per pigeonhole it is perhaps evident that they would not necessarily have been in conflict. All of McDougall's theoretical system, *e.g.*, might be fitted into the classification-inference bracket, as might any similar compatible system. And presumably all of theoretical behaviorism will fit under experiment-inference. Properly, then, no item in any one pigeonhole conflicts with any item in another pigeonhole, though a complete parallelism of differing items might hold between either of the classification-inference systems and the experiment-inference system. Any apparent conflict will then be, it seems to me, on account of the behaviorist's extension of claims beyond the problems he admits as real. The issue therefore becomes philosophical: Which view is the proper framework for psychological analysis? But, to answer that question now is to limit the possibilities and become scholastic. Accordingly, I prefer to let each system develop independently, and to seek a more general framework.

On the other hand, McDougall's 'propensity,' and the 'trait' of Allport do represent conflict insofar as both are classification-inferential. Here again, though, we find different modes of attack. The 'trait' is specific, 'propensity' general. However, it would seem that 'common trait' and 'propensity' are directly aimed at the same formal attribute, *i.e.*, are logically equivalent. Once this is established, we have common ground between the two systems.

VI. Psychological Theory

In view of what has preceded, we may treat psychological theories more explicitly if not yet adequately. An important obstacle lies in the fact that the immediate nature of psychological phenomena implies fore-knowledge of just those

things we strive to render more comprehensible and simple by individual or collective modes of treatment. It is neither possible nor desirable to outlaw such knowledge (A_2), but it is both desirable and possible to demonstrate the inherent assumptions and consequences, both implicit and explicit.

This means that psychological systems require a double examination: for their analytic features, and also to discover what common-sense factors are open to misunderstanding and opposition. There is a danger that one psychologist shall also decide what others ought to say and do; it should suffice for the present that he attempt to find out what they have in reality done. We must first of all seek to know their basic presuppositions both implicit and explicit, *i.e.*, their points of vantage in a scale of possibilities ranging from philosophy to natural science. Their assumptions limit us in two directions: First, to question them begs the question, and second, to anticipate anyone's conclusions more narrowly than is deducible involves new assumptions. In either instance we may point out phenomena he has not treated, and that is all; within his position he is impregnable, and beyond it he says nothing. (Insofar as his assumptions are consequences from other fields, the systematizer should be prepared to extend the grasp of his assumptions to include such well-substantiated new material in those fields as can be suspected to influence psychology, if he hopes to avoid the risk of stagnation.)

In addition we require to know what he sets forth as his pretensions: his scope of specialized interest, and how far he goes beyond it; what data he treats, and by what generalizations. It is not sufficient that his system be deductively sound; it must apply to unabstracted reality without any major lapses. For no system with empirical implications can escape the 'brute stubbornness of fact.' In some cases it is the part of wisdom, though not of system, to retain what seem to be incompatible relations. When two hypotheses are directly opposed in the light of current data, we cannot make a valid comparison until new facts are turned up. To turn once again to an overworked example: In physics the wave and quantum theories coexist. Eventually some new theory may comprehend them, just as many present generalizations

have resolved former conflicts, *e.g.*, the heliocentric view of the solar system.

The more fully we consider these things the better we can criticize. At present, however, I feel able to go only so far as my present schema indicates. It is an avowedly imperfect instrument; at most it can enable us to break down individual systems and discover the main features they hold in common when preconceptions have been discounted.

For present purposes it is not necessary to decide on the absolute value of this or that theory. In fact, so far as two theories cover the same ground, they must employ inter-translatable generalizations, differing only as to their place in some system and/or as to technical fertility; for the most part, I suspect that no unitary theory can yield more than one outstanding generalization-technique contribution. Hence, if the comparison of present systems might be made on some such basis as this essay has indicated it is conceivable that a growing region of agreement would appear between systems.

The most fruitful immediate step toward agreement that suggests itself to me is to review critically the general terms used in psychology, stating the scope of their usefulness as regards subject-matter, problems and goals. If those men who are especially interested in the various schools and systems for psychology were to phrase their own positions analytically, that first step might be taken. Alone it would not, however, furnish sufficient basis for criticism. We should next have to align each generalization-problem-method complex with its competitors. And, as such comparisons become inadequate or incomplete, to rephrase our statements in terms of further more refined analysis. What might result finally would be a grand resynthesis of everything that has hitherto been treated in psychology, with the ideal of settling upon a simple set of coordinates (C) and postulates (A, D, and P), and a general agreement upon some basic theory, from which to make side trips, as has been done with Euclidean geometry, Newtonian physics, or Copernican astronomy, all of which have been much refined, but still retain their essential character.

[MS. received May 23, 1940]

UNITY IN PSYCHOLOGY: A SURVEY OF SOME OPINIONS

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A long-time view of the course of psychological theorizing shows a succession of particular problems commanding more or less widespread interest. Frequently these cannot be labeled as being mere passing fancies or solid contributions to psychology until after they have passed and we can view them in a larger perspective. Some of them leave their mark while others disappear altogether. In psychology today there are several such problems of special interest: Psychometrika is fast becoming a new word in the vocabularies of a good many psychologists. E.S.P. swept a large number into its net but most of them squirmed out as rapidly as possible. Experimental neurosis has aroused a good deal of activity and discussion on all sides. Apparently it will be some time yet before psychology or Americans get tired of this topic of 'neurosis.' Operationism is a current problem of importance and popularity and one toward which we are gradually improving our orientation. There is now an increasing number of articles in the literature that revolve about Hull's formulation of 'scientific psychology.' His postulational system is an interesting application today of an old and venerable scientific method. Allport has given impetus and definition to the new movement which he calls the 'psychology of personality.' There is no doubt that interest in this problem is spreading rapidly. Probably our experiments in writing introductory text-books should be included in this list. In addition to these problems and occasioned by their manifold and conflicting interests, the general question of unity in psychology is receiving a good deal of attention in current

[1] Research paper No. 708. Journal series, University of Arkansas.

psychological literature. This is the problem I propose to discuss.

In the past unity was thought to be largely a function of subject matter. The early development of schools and their continued survival is a reflection of this attempt to unify psychology. Formerly we could pretty well depend on the unity and similarity of interest by members of each particular school. But today the rigidity of this distinction is breaking down among experimental and theoretical workers; the traditional schools are losing their influence as an integrating factor. Psychologists are looking for new and different ways of maintaining and stimulating greater unity in their science. By unity we simply mean a condition whereby the different results and theories of psychologists can show a meaningful relation to one another. Little can be said in support of a science that dissipates its energy in fruitless controversy arising out of mutual misunderstanding.

To what unifying principle have our contemporaries turned? Many have become interested in the question of methodology. Today the stress is on methodology; yesterday it was on subject matter. This is not a hard and fast distinction (though some would make it so, *e.g.*, Pratt, **8**), but it is one that is becoming more evident. Contemporary psychologists justify this new approach from either one of two positions: First, they have let their method define their subject matter. Some of the neo-behaviorists, notably Hull, appear to have taken this position. The other point of view is more extreme and regards the problem of subject matter as insoluble and probably unimportant since the definition of any science is in terms of its methodology. A specific example of this attitude is our treatment of intelligence. We have given up trying to define it and are simply trying to measure it. Pratt's recent book, *The Logic of Modern Psychology*, is a systematic reflection of this point of view.[2]

In either case these two attitudes imply that the road to

[2] Karl Pearson reflects this attitude in his *Grammar of Science* (7): "The unity of all science consists alone in its method, not in its material." But we must not take this sentence too literally as it is a statement intending to show the wide applicability of the scientific point of view to human problems.

unity lies in our adopting a common methodological approach. Methodology is but a means to a larger end. Titchener's insistence on a common method is probably the very factor that set the narrow bounds of his psychology. Logically subject matter is prior to method. Agreement as to method may be an advantage where agreement is possible. But I cannot understand how psychologists can employ a single methodology and thereby assume they are putting our science on the sure road to the solution of its problems. The inevitable result of such a position is immediately to limit psychological research to those problems which are amenable to this methodology. I would consider this a form of begging the question. Basically, the subject matter of psychology is human behavior and our problems arise irrevocably from conditions found when human beings live and seek adjustment in the world in which they live. There is no single methodology that we can use to attain this goal. Allport's (1) metaphor is apropos, "Methodism as the sole requirement of a science means that all the faithful crowd onto a carpet of prayer, and with their logical shears cut more and more inches off the rug, permitting fewer and fewer aspirants to enjoy status." Titchener used but one method and our fundamental objection to his psychology is the limitation of its subject matter.[3]

Three specific examples of the current search for unity through methodology are operationism, postulational psychology, and that which we sometimes call mathematical psychology. Of these, operationism has attracted the greatest interest. Operationism is not a method in the same sense that statistics, conditioning and the like are methods. But it is a scientific orientation which determines and limits one's way of psychological thinking. In a literal sense operationism is inadequate as a unifying principle because it does not lead to the creation of new theories and interpretations; nor does it encourage us to interpret our experimental results in terms of the larger theory out of which the problems arose.

[3] Titchener is gone and perhaps his particular psychology will follow him into history, but there is one Titchenerian principle that should long remain in psychology, and that is his never compromising attitude toward making psychology a distinct, consistent, and separate science with a contribution all its own.

In his 'scientific system,' Hull has given us the framework of a methodology by which he proposes to investigate all legitimate psychological problems. To some of us who are not so 'scientifically minded' such a proposed extension may involve a good deal of question-begging. So far Hull has applied his methodology to but a few specific problems of behavior and to some definitions.

Many psychologists emphasize the use of mathematics in psychology. Their credo is: Psychology to be scientific must be quantitative. Their logic seems to be as follows: In truth there is unity; numbers do not lie; therefore by presenting psychological facts in numerical form we shall have truth and the unity which it brings. I would object to this as I would to any point of view that adopts but one method of solving problems. Not only is it a dogmatic and arbitrary position, but it implicitly assumes that the important problems can be solved by this particular technique. We may agree with the psychometrician that many problems can be quantified and many that are now qualitative may eventually become quantitative, but we have no reason to believe that *all legitimate* psychological problems can and must be eventually quantified. Another criticism arises from the fact that this mathematical technique is a method. It is simply a way of describing conditions. The problems to be solved and the interpretations and evaluations that are made are always qualitative, by definition. Numbers are meaningless without interpretation. We must remember that statistics do not make knowledge; they merely describe in an abbreviated and conventional form the results of the investigation. If there is any unity to come from the use of mathematics in psychology it is certainly not from the numbers themselves but from the interpretations and use that are made of them. The quantitative method is not, therefore, in itself, any assurance of unity in psychology as a complete science.

These methodologies may display a certain amount of integration and theoretical consistency. But they are methods and presumably have been formulated to overcome the difficulties arising from research programs and experimental tech-

niques. It is a logical and systematic error when these methodologies are expanded and emphasized as the integrating and unifying factor for all the problems of psychology. We know far too little about the universe in general or human nature in particular to assume that one methodology will apply to all legitimate psychological problems. A particular method reveals a particular aspect of the object of study. The problem of unity in psychology cannot be solved by making it an incidental result of the use of a particular methodology. Unity is a distinct and separate problem from that of methodology. We seek unity so we can give meaning *to* rather than take meaning *from* the results of a methodology. Panaceas for the problem of unity are non-existent. The best we can do is to recognize it as a separate problem which psychologists are still trying to solve.

Recently several of our leading psychologists have published papers that deal with various aspects of the problem of unity. Conklin (**5**) approached it from the point of view of administration; pointing out that administrators in the field of psychology are meeting serious problems due to the lack of organization and unity in their profession. Dashiell (**6**) has been more optimistic in his paper. For instance, he has interpreted the methodological emphasis of the operationists, Hull and Lewin, as indicating a growing unity in psychology. He specifically describes several other rapprochements and in general would turn our "attention to harmonizing trends, to some of those ways in which the subject matter of psychological research is leading to more mutual understanding and cooperative effort" (p. 21). Nevertheless, we may wonder if the disruptive conditions are not still dominant. Some of these divergent trends may be: the new and rather specific interest in applied psychology, the restriction to the physiological point of view, limitation of method to quantitative techniques; breaking up of psychology into special courses in other departments, *e.g.*, business, social, and educational psychology. In fact it seems that any specialized interest in psychology can be taken as a divergent trend. Gordon W. Allport's stimulating paper (**1**) offers several impli-

cations for a unified psychology. To quote, "My plea, therefore, is that we avoid authoritarianism, that we keep psychology from becoming a cult from which original and daring inquiry is ruled out by the application of one-sided tests of method; that we come to evaluate our science rather by its success in enhancing—above the levels achieved by common sense—our powers of *predicting*, *understanding*, and *controlling* human action. As an aid to progress I have tried to strengthen the case for research upon complex patterns of human mental organization, frames of reference, the subjects' point of view, and the act of understanding" (p. 26).

These reports are among the many that reflect our current interest in the problem of unity. The answer to the question, "What is a unified psychology?" will, however, always reflect the bias of the individual. The attitude of the writer is that unity in regard to the fundamental theoretical problems of the science is the only real unity that is important. For unity in this basic relation does not necessarily exclude diversity and complexity. Our object of study, being human behavior, has many aspects and avenues of approach. Each of these must be recognized and appreciated. Lack of unity results when we fail to see the inherent relations between the various approaches to the study of human behavior. Carr and Kingsbury (**4**) have recently pointed out in a very specific way the necessity and possibility of systematically integrating the various approaches to an understanding of the 'psychological individual.' Such a position contributes to unity in psychology.

I cannot speak for other sciences but it does seem that many psychologists have been somewhat naïve in expressing their American tradition of individuality. Tactics often used are: coining new terms and phrases, or applying old concepts to radically different conditions.[4] Such practices lead to confusion within psychology and should be discouraged in the interest of a unity of mutual understanding, if not agreement.

We may make certain attempts at unification in matters

[4] There are times when such changes should be made, but always critically and with care to make clear the necessity of the change.

of administration, but this would be superficial with respect to the basic function of a science. Surely we should not want to make psychology unified solely as an applied technology. However, we are aware that the socially felt needs of the times should be a factor in giving unity and direction to this *human science*, par excellence. Ultimately any science or academic discipline must have some utilitarian value though this need not be its immediate objective.

Usually a younger generation resents feeling so old-fashioned that they must admit the work of a previous group to be superior to that of their own contemporaries. But with respect to this problem of unity in psychology I cannot help but think that the former emphasis on subject matter was logically and pragmatically more sound than our present stress on methodology. Psychological problems arise out of conditions of living and not from an experimental laboratory. As Carr points out in the *Psychologies of 1930*, "One of the distinct differences between Structuralism and Functionalism is that the latter was extended to include utilitarian values" (**3**, p. 66). In fact he says, "A functional psychology is thus primarily interested in correlating all adaptive forms of behavior in all possible ways. It suffers from no taboos in this respect." In a final note at the end of their recent textbook, Boring, Langfeld, and Weld (**2**, p. 627) summarize psychology as ". . . a description of man adjusting himself to his environment and his environment to himself." These are broad statements. But they agree in placing the problem of psychology in the functional relationship between man and his environment. As a definition of subject matter this attitude is of scientific and theoretical importance because it gives us a common point of reference, namely, the adaptive behavior of normal human beings. It is from this that psychology starts. Research should be in the direction of the solution of these problems. As psychologists we are not trying to discover 'vectors', 'means-ends-readinesses,' rational learning curves, postulates, and the like, *per se*. These should be but means to the end of trying to understand the nature of man and his behavior, not alone in the abstract, but specifically, here and now.

To attain this goal it may be necessary to employ many different modes of attack. We hold no prejudice against the use of any method, *per se*. Introspection, statistics, conditioning, operationism, rationalism, and any other *instrument of study* may make its appropriate contribution in psychological research. We can find unity in psychology only insofar as we can make an integration between these various procedures. In actual practice this seems to be the position that most American psychologists have taken. The question is: Can a unified psychology develop from such an eclectic attitude? It is the position of the writer that it can if certain cautions are observed.

The most important of these is the integration of fact with theory. Experimentation in its larger meaning is the *sine qua non* of the scientific method and psychology owes much to experimental results. But many workers seem to confuse mere experimentation with progress in the science itself. An indiscriminate accumulation of experimental facts is particularly meaningless in a science as complicated and disjointed as contemporary psychology. No experimentalist has completed his job until he has made his results meaningful in terms of some theoretical frame of reference. There is no point in running around the bases if you do not touch home plate. Where differences exist in basic theory, especially with respect to subject matter, there is little hope for agreement or unity. But where there is a common starting and stopping point in theory, experimental facts will never be completely isolated in meaning.

We have thus taken the position that the first challenge for experimental or rational research is not one of methodology, but one relating to problems of subject matter, meaning simply the adaptive behavior of man. If it were possible to find agreement in this initial phase of psychological research, subsequent experiments and theories would rarely be completely divorced or unrelated to each other. Experiments and theories having started from the same place and being ultimately directed to the same theoretical goal, there would always be a fundamentally unified psychology. The most

telling invective that could be cast at any experiment would be that it was 'pebble-picking.' Pebble-picking is an experiment which presents a specific problem together with a solution within the limits of a single study. There are no true psychological problems as simple as that. *So far as the unity of psychology is concerned the most important part of any published experiment should be the interpretation or discussion wherein the specific results are given meaning and importance with respect to some larger problem.* Until the experimenter can do this he is not truly a research psychologist. Examples of pebble-picking experiments are to be found in all branches of psychological investigations. Too frequently comparative psychologists become zoologists or makers of ingenious analogies between human and animal behavior. Human experimentation too often becomes a display of remarkable apparatus and submissive subjects. Psychologists must guard against becoming more nearly technicians than thinkers.

We have repeatedly emphasized the fact that psychological problems arise from every-day conditions of behavior. This is an all-inclusive field with almost unlimited possibilities for finding experimental problems. But the psychologist should not be tempted and led astray by this myriad of problems. His first job is one of classification and systematizing. He should construct an outline, so to speak, of this behavior that is so complex in its 'natural state' and which by tradition has become so mystified. His experiments should be designed in terms of this outline. They are worthwhile experiments so far as they show the validity of the theoretical construct or ways in which it might be altered and improved.

Summary

This paper presents a brief survey of a renewed interest in an old problem of psychology, namely, that of unity. Recently several psychologists have written on this problem though each conceived the question in a somewhat different light. A good deal of this contemporary discussion has emphasized methodology as the unifying factor. The objection is raised that any particular methodology is inadequate in

this respect since it presents but one approach to the object of study. Unity in psychology must be considered as a separate problem which we are still trying to solve and not as the incidental result of the use of a single methodology. We shall find unity only insofar as we can integrate the results of the different methods used in psychology. As a matter of specific procedure each problem of research must be conceived, directed, and interpreted in terms of some fundamental frame of reference. Without this procedure the chief criticism of any experiment would be that it is 'pebble-picking.' Such a specific, fact-finding experiment makes integration difficult and frequently leads to wrangling within any science. Especially is this true in psychology which is still heavily dependent on fundamental theoretical formulations to guide its experimental studies.

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[MS. received June 25, 1940]

THE EMPIRICAL DETERMINATION OF AN ÆSTHETIC FORMULA

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I. Previous Investigations

Several attempts have been made in the past few decades to determine the pleasure derived from various types of æsthetic objects by reference to their numerical or geometrical characteristics. Such attempts as those of Emch (**5**), Birkhoff (**2**), and Rashevsky (**13**) may be instanced here. This desire to find a 'formula' for beauty is of course nothing new; Evans has traced it back to the time of the French Revolution (**6**, p. 51), and it would even appear as if we should place the beginning of this quest in the time of Pythagoras, who was perhaps the first to connect music and number.

However, it is only recently that this type of study has entered into an experimental phase. These experiments have mostly been connected with Birkhoff's *Æsthetic Measure* (**2**). Birkhoff maintains that our pleasure in any work of art depends on two variables: the amount of Order ('*O*') and of Complexity ('*C*') in the object. These are measured in various ways for different classes of objects, but all classes obey the general formula:

$$M \text{ (the amount of pleasure derived)} = O/C.$$

Birkhoff himself has worked out his formula in detail for polygonal figures, vases, poetry, and music. The theory on which the formula is based has been criticised by Davis (**4**, p. 233), who finds that "we are forced to conclude that the a priori evidence in support of the æsthetic measure formula is insufficient and sometimes dubious."

Several investigators have tested the predictive value of

the formula. The results to date are given in Table 1; it will be seen that the correlations between the actual rankings and those given by the formula are positive throughout; there is, however, a great deal of variation in the size of the coefficients, the largest being .70, and the smallest being .05.

TABLE 1

Investigator	Material	Subjects	Correlation with A. M.
R. C. Davis	10 Polygons	162 Students	.11
R. C. Davis	10 Polygons	55 Art Students	.05
F. W. Swift	45 Polygons	6 Laymen	.53
F. W. Swift	45 Polygons	3 Art Students	.22
F. W. Swift	15 Polygons	6 Students	.16
C. M. Harsh et al.	26 Polygons	30 Students	.34
H. J. Eysenck	32 Polygons	14 Observers	.53
H. J. Eysenck	32 Polygons	14 Observers	.62
H. J. Eysenck	15 Polygons	12 Observers	.48
R. C. Davis	10 Poems	63 English Students	.55
H. W. Miller	7 Lines of Poetry	16 English Students	.11
H. W. Miller	7 Lines of Poetry	5 Students	.08
J. B. Parry	19 Lines of Poetry	14 Observers	.34
H. W. Miller	7 Nonsense Lines	5 Students	.68
A. Schnittkind	15 Vases	5 Students	.23
A. Schnittkind	15 Vases	8 Observers	.16
A. Schnittkind	15 Vases	7 Art Students	.09
E. Fischer	10 Chordal Sequences	15 Observers	.05
E. Fischer	5 Unharmonized Melodies	145 Observers	.50
E. Fischer	5 Unharmonized Melodies	59 Observers	.50
E. Fischer	5 Unharmonized Melodies	19 Observers	.70

The investigations by F. W. Swift, H. W. Miller, A. Schnittkind, and E. Fischer are reported by Beebe-Center and Pratt (1). J. B. Parry's investigation has not as yet been reported elsewhere. My own results were the incidental outcome of the investigation described below. The reference to the article by R. C. Davis has already been given.

The most important of these investigations is undoubtedly that carried out by Harsh and Beebe-Center (12). Not content with simply finding the amount of agreement between the formula and the rankings of their subjects, they went further and attempted to discover the factors which influenced the subjects' choice. Using a modified form of Thurstone Factor-Analysis, they found four main factors, which they identified as: (1) Liking for smoothness of contour; (2) Liking for simple, regular geometrical figures; (3) Liking for sym-

metry, mainly rotational and diagonal; (4) Liking for odd points.

They suggest developing a formula from these four factors, taking into account the extent to which a given observer values each of the judgmental factors. My own approach has been rather different, and it must be said at once that my results agree only partially with those reported above. An attempt to account for the differences will be made at the end of this article.

II. The Present Research

The material used in my main investigation consisted of sixty-four polygons, photographed in black and white from Birkhoff's tables. The principle according to which the polygons were selected was to avoid those which had too obvious associations, such as the swastika or the Jewish star. Otherwise, an attempt was made to include at least one example of each of the various kinds of polygons.

The polygons were divided into two sets, of thirty-two each. The average scores of the two sets by Birkhoff's formula were .39 and .31.

The same fourteen observers were asked to rank the polygons in each of the two sets in order of preference, using a fixed distribution which closely approximated the normal distribution curve. The observers, seven women and seven men, included artists, students, professional men and women, teachers, stenographers, and psychologists. None of them were familiar with Birkhoff's theories.

The rankings in each of the two groups were correlated, and the resulting tables of correlations submitted to a statistical analysis. An effort was made to show that the principal factors in the two tables were identical, and a determination of these factors was attempted.

In order to construct a formula on the basis of the results of these experiments, contingency correlations were worked out between the average order of the polygons, and certain geometrical characteristics which it appeared likely were partly responsible for their relative popularity. The squares

of these contingency correlations were then used as weights in a regression equation of the form:

$$M = b_1x_1 + b_2x_2 + b_3x_3 + \cdots + b_nx_n,$$

where the dependent variable, M, is the average judgment; x_1, x_2, $x_3, \cdots x_n$, the independent variables, are the chosen characteristics of the polygons; and b_1, b_2, $b_3 \cdots b_n$ are the weights.

The formula found in this way was tested by correlating the order of the polygons given by it with the average order of the polygons in each of the two sets, and also by applying it to the results of another test.

III. Results and Discussion

In an endeavour to test the stability of the factors from one matrix of correlations to the other, Burt's symmetry criterion was used. This criterion rests on a theorem of frequent use in quantum-mechanics; namely, that the principal axes of two symmetrical matrices, R_1 and R_2, and therefore their principal factors or components, will coincide if, and only if, the two matrices commute, *i.e.*, if

$$R_1R_2 = R_2R_1 = (R_1R_2)',$$

and therefore if the product is itself symmetrical.

When this test is carried out, it is seen that the product-matrix approaches very closely to symmetry; as closely, indeed, as one could reasonably expect in view of the high standard errors. There is no considerable deviation between the sums of the rows and of the corresponding columns; on the average, the deviations are only 3 per cent of the mean totals of the rows and columns, the highest being no more than 5 per cent. Hence we conclude that the principal factors in the two matrices are identical.

Most of the correlations in the two matrices are positive, only ten in one and fourteen in the other being negative, out of ninty-one. The highest correlations in the two matrices are .69 ± .09 and .74 ± .08; the highest negative correlations

in both matrices are $-.59 \pm .12$.[1] The averages of the correlations in the two matrices are .27 and .25.

These two values enable us to calculate, by means of a formula which I have shown elsewhere to be applicable to data of this kind, what the correlations of the average orders of the polygons as given by the fourteen observers in the two tests would be with the 'true order,' as given by the whole population of which our observers are only a sample (**7**, p. 652). These correlations work out at .92 and .91.

Two factors were extracted from each of the two tables of correlations; the first one in each case being a general factor, having positive saturations throughout, the second one being bipolar in each case, *i.e.*, having positive and negative saturations in roughly equal numbers. The general factor accounts for 32 per cent and 31 per cent of the variance, the bipolar factor accounts for 15 per cent and 11 per cent.

In the factorial analysis of correlations between persons, Davies has shown that the second factor to be extracted is nearly always statistically insignificant (**3**). Employing the same criterion which she used (she tested the residuals on which the second factor was based by Fisher's test of the difference between the theoretical and the actual correlations, expressed in terms of their inverse hyperbolic tangents; $z = \tan h^{-1} r$), the second factor was found to be statistically significant in both tables.

Leaving aside for the moment the first, general factor, we can easily identify the second, bipolar factor by examining the polygons on which there is a great difference of opinion between those subjects with high positive and those with high negative saturations. It would appear that the opposition indicated by this factor is between simple, regular polygons with few sides on the one hand, and complex, not so regular, many-sided polygons on the other. The square, equilateral triangle, rectangle, and diamond are the outstanding examples of the 'simple' type of polygon; some of the polygons with

[1] Following Fisher's advice (**11**, p. 46), I have given the standard error with each correlation, rather than the probable error.

rotational symmetry, and complicated designs such as the Greek Cross, are representative of the 'complex' type.

As regards the general factor, it would appear to be related to another factor which I have discussed elsewhere (**9**). In that article, a general factor of æsthetic appreciation was found to run through eighteen tests, each of which involved the ranking of between ten and thirty pictures of such diverse material as portraits, devil masks, mathematical curves, statues, pencil sketches, and pottery. This factor was called 'T,' and was shown to be correlated with factors derived from correlations between rankings of simple colours and of odours.

A test of this 'T'-factor was given to the subjects of the present experiment, and the correlation found between their scores in the test, and their saturations for the general factor in the two polygon tests is definitely significant; it is .68 ± .19. If the 'T'-test were a perfect test of the 'T'-factor, we might say that the polygon-tests are saturated to that extent with 'T,' in a similar way as an intelligence test is said to be saturated with '*g*.'

As regards the nature of this factor, an indication was given in my article (**9**) as to its possible explanation; I have worked out this suggestion in some detail along the lines of Gestalt theory, and hope to publish it shortly. At the moment, however, our interest lies mainly in those characteristics of the polygons which are correlated positively or negatively with the general factor. Twelve of the most important of these are given in Table 2, together with their contingency correlations with the average order of the polygons.

Some of the terms used in this table are unambiguous; others may stand in need of elucidation. *Vertical and Horizontal Symmetry* are self-explanatory; it should be noted, however, that in this article both these types of symmetry are held to apply only to compact figures, *i.e.*, figures without re-entrant angles, and to semi-compact figures, *i.e.*, figures

[3] This bipolar factor of 'complexity versus simplicity' does not seem to be restricted to polygons; in analysing the rankings of thirty-two poems by fourteen subjects a similar factor was found, dividing those who preferred a regular rhyming scheme, well-marked rhythms, and simplicity of meaning, from those who preferred more experimental, irregular rhyming schemes and rhythms, and greater complexity of meaning.

TABLE 2

Basis of Judgment	Coefficient of Contingency
(x_1) Vertical or Horizontal Symmetry	.71
(x_2) Rotational Symmetry	.69
(x_3) Equilibrium	.51
(x_4) Repetition	.45
(x_5) Compact Figure	.37
(x_6) Complexity Six or more	.33
(x_7) Both Vertical and Horizontal Symmetry	.31
(x_8) Pointed Top and/or Base	.20
(x_9) Complexity Three or more	.10
(x_{10}) Complexity Two	−.17
(x_{11}) Re-entrant Angles	−.52
(x_{12}) Angles close to 90 degrees or 180 degrees	−.63

having niches the area of which is less than one-sixth of the total area of the polygon. *Rotational Symmetry*, on the other hand, only applies to polygons which are neither compact nor semi-compact. A polygon has *Equilibrium* if the centre of area lies between two vertical lines erected on the extreme points of support on the horizontal base, but at a distance from either of them exceeding one sixth that of the total horizontal breadth of the polygon. It is also said to have equilibrium if it has a pointed base and vertical symmetry. A polygon has *Repetition* if it can be divided into two parts in such a way that one part is the mirror image of the other, while differing from it in size. *Complexity* is defined as the number of non-parallel sides of the polygon. The other terms used will not present any special difficulties.

As explained previously, we now use the squares of the correlations as weights in a regression equation, in order to obtain an empirical formula for the pleasure derived from each polygon. When a few slight adjustments are made, necessitated largely by the fact that one or two of the bases of judgment occur only very rarely in the polygons, and their influence is consequently not represented quite accurately by the coefficient of contingency, the following formula is obtained

$$M = 20x_1 + 24x_2 + 8x_3 + 7x_4 + 5x_5 + 3x_6 + 3x_7 + 2x_8 + 1x_9 - 2x_{10} - 8x_{11} - 15x_{12}.$$

The accuracy of this formula can be tested by correlating the average orders of the two sets of polygons with the orders

given by the formula. The coefficients of correlation are respectively .91 ± .03 and .88 ± .04; that is to say, our formula accounts for approximately 80 per cent of all the causal factors entering into the preferential judgments of the polygons included in the two series.

It will be remembered that in view of the average intercorrelations of the subjects' rankings, we concluded that the correlation of their average order with the 'true order' would be about .90. That is to say, even a perfect formula for polygonal forms could not predict the average arrangement more closely than our own formula does. About 20 per cent of the factors entering into the average judgment are chance factors; the other 80 per cent are accounted for by the formula.

Another experiment was carried out in order to see how well this formula would work if applied to the rankings of a different group of observers. Twelve subjects were asked to rank in order of liking fifteen polygons from Birkhoff's selection, five of which had not been used in the two preceding experiments. The rankings were correlated, and two factors extracted from the resulting table. The first, general factor, accounts for 26 per cent of the variance, the second, bipolar factor, for 20 per cent. Again the bipolar factor marks the opposition between liking for 'simple' and for 'complex' polygons.

The average of all the correlations in the table is only .17; that is to say, the correlation of the average order with the 'true order' would be .85. The average order agrees with the order given by the formula to the extent of .84 ± .08. Again nearly all the causal factors, except the chance factors, are accounted for by the formula.

Two points remain to be discussed. One is the relation of the factors found in our analysis to those found by Harsh and Beebe-Center. The other is the rôle to be assigned to the bipolar factor in a formula such as the one given above.

Clearly the results of our analysis are not identical with those obtained by Harsh and Beebe-Center. It may be surmised that this difference is largely due to the method of analysis adopted; Harsh and Beebe-Center would appear to

accept Thurstone's theory of primary structure, while the present writer, in conformity with most of the leading British factor-analysts, cannot see his way to accept this theory (**8**). The fact that a correlation was found between the first or general factor extracted in our analysis, and the 'T'-factor, would seem to point towards some such analysis as ours as being perhaps nearer the truth; but the whole question is not advanced enough at the present moment to be capable of definite settlement.

As regards the bipolar factor, its presence certainly raises a number of problems in connection with the formula. Factors of this kind are often very important, and at times even overshadow the influence of the general factor (*cf.* **10**, for an investigation into bipolar factors). However, by their very nature the two different poles of these factors more or less cancel out, and therefore do not greatly affect the formula.

This is only true when we deal with average judgments. When we attempt to predict the order of liking of any individual, then it becomes necessary in many cases to take into account the bipolar factor. This can be done by extending the procedure used in finding the formula for the general factor to characteristics which differentiate the two poles of the bipolar factor; these can then be weighted for every subject by the saturation (positive or negative) of that subject for the bipolar factor.

Work is in progress at the moment to extend the formula in this way, and it is hoped that substantial progress can soon be reported. An effort is also being made to discover the factors active in the judgments of vases and poems, and to derive formulæ for these two groups of æsthetic objects in a manner similar to that used in the present investigation.

IV. Summary

In an endeavour to test Birkhoff's formula for polygonal forms, and to derive a new formula from the actual experimental results, some seventy polygons were judged by altogether twenty-six observers. Two factors were found to account for all the correlations within the limits of the stand-

ard error: a general, positive factor, which correlated significantly with the 'T'-factor, and a bipolar factor, which divided the people preferring the simple figures from those preferring the complex figures. (A similar bipolar factor had previously been found in judgments of poetry.)

A formula was developed by correlating various possible bases of judgment with the average order of the polygons, and by using the squares of the correlations as weights in a regression equation. This formula was found to account for all the non-chance factors operating in the judgments of the observers in three different groups of polygons.

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[MS. received July 5, 1940]